



Health
Canada

Santé
Canada

Evaluation Group

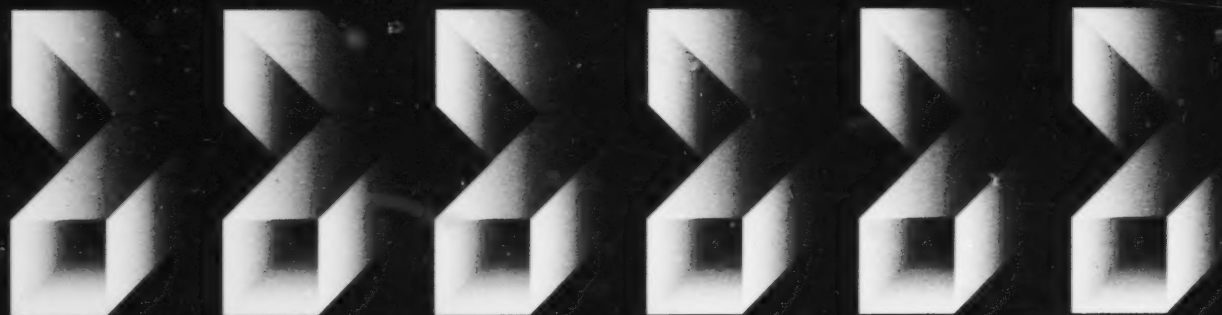
**Strategic Planning
and Review
Directorate**

**Policy and
Consultation Branch**

Groupe de l'évaluation

**Direction de la
planification stratégique
et de l'examen**

**Direction générale des
politiques et
de la consultation**



**A Review of Evaluations of Anti-Smoking
Interventions in Countries Other Than Canada**

March 31, 1995

Canada

MISSION STATEMENT

The Evaluation Group helps Health Canada serve Canadians better by providing objective information and advice about the effectiveness of departmental activities and services.

ÉNONCÉ DE MISSION

Le Groupe de l'évaluation aide Santé Canada à mieux servir les Canadiens et les Canadiennes en offrant une information et des conseils objectifs sur l'efficacité des activités et des services du Ministère.

**A REVIEW OF EVALUATIONS OF
ANTI-SMOKING INTERVENTIONS
IN COUNTRIES OTHER THAN
CANADA**

**Submitted to
PROGRAM EVALUATION DIVISION
HEALTH CANADA**

**By
THE ONTARIO TOBACCO RESEARCH UNIT**

March 31, 1995

Executive Summary

The Program Evaluation division of Health Canada has commissioned a critical review of evaluation methodologies for tobacco control interventions in other countries. The review was designed to assist the Program Evaluation Division in preparing the evaluation of the Tobacco Demand Reduction Strategy (TDRS). This report is to be used as a basis for selection of optimum approaches.

This report offers an analysis of Tobacco Control Program interventions with an emphasis on evaluation methodologies used in other countries. The analysis focusses on a critical assessment of methodological approaches used to study the effectiveness of the Interventions.

The level of analysis and approach to the review of intervention areas varies by the amount of available literature and information available for review, the complexity and scope of the intervention area and the focus of the analysis. The project team has focussed on evaluation methodology issues as stated by the contract terms of reference. A detailed analysis of intervention programs and their outcomes awaits future study within a new mandate.

The project team was comprised of principal investigators from the Ontario Tobacco Research Unit (OTRU) who conducted extensive literature reviews on evaluation methodologies applied to specific tobacco control interventions identified by Health Canada. These intervention areas included: Community based Initiatives, Worksite based interventions for Blue Collar Workers, School Based Interventions, Media Campaigns, Tobacco Prices and Their Impact on Tobacco, and Non Price restrictions.

The report offers a summary analysis of the types of evaluation studies available for analysis which includes needs assessments, implementation evaluation, process evaluation, outcome or impact evaluation, comprehensive evaluation, formative and cost benefit evaluations. The "introductory" analysis also provides an overview of available evaluation methodologies and provides a series of methodological issues/questions in the evaluation of program interventions.

Our review of **Community Based initiatives** addresses evaluations of community-based cessation programs which were directed toward women, low income groups, people with identified smoking related illnesses, as well as the community as a whole.

The chapter first describes issues which need to be considered in planning an evaluation for community-based initiatives. It then describes the types of approaches which have been applied at the community level to help smokers stop smoking. Several examples of different types of evaluations are provided which demonstrate how different projects have addressed the evaluation of these approaches. A bibliography of further examples of programs is attached.

Any analysis of community based interventions must remain cognizant of the scope, factors and unique characteristics inherent in a specific community. Our analysis has identified the strengths and weaknesses involved with each identified methodology. In spite of these limitations, the following general conclusions can be made:

The community-based comprehensive programs exemplified by the CVD risk reduction

projects and the COMMIT trial, provide a firm foundation for future evaluation of community level initiatives. They have shown us the level of intensity and length of intervention needed to produce changes in risk factors and reduction in morbidity and mortality. From these projects, we have learned about the costs of different kinds of evaluation. We have also learned that it is possible to create a standardized protocol which can be accepted and implemented in different communities at levels which make comparisons possible.

It is clear that evaluations which contribute to our understanding of the process of successful community-based programming are needed as essential next steps. New methods or new combinations of old techniques are needed and the expertise for this may not be readily available. Health Canada is well situated to bring such individuals from different disciplines together in order to build this area of technical assistance throughout Canada.

We need to address the fact that many community-based studies have found that the secular trends and measurement impacts have produced changes in comparison communities that lead to insignificant differences between intervention and comparison communities. This means that evaluations which compare communities need to be selected with care and justified accordingly.

Worksites may offer an ideal context in which to implement comprehensive smoking interventions. Not only do these efforts afford protection from environmental tobacco smoke (ETS) to nonsmokers, but, also, assistance can be given to those who want to quit, and the establishment of nonsmoking social norms can be enhanced.

Two basic types of interventions have been reported in the literature: policy changes, either at the individual work site level or at the jurisdictional level, and cessation programs. Smoking policies include restrictions or bans on smoking at the workplace, refusals to hire smokers, certain incentives for smokers to quit, negative sanctions against workers who do not quit, and the removal of cigarette vending machines from company premises. These policies are enacted for several reasons; to protect non smokers from the health effects of environmental tobacco smoke); to reduce potential fire and other damage from smoking; to increase worker productivity and reduce time lost from work. Such policies, however, may also impact on the smoking behaviour itself. Smoking cessation programs in the workplace are specifically designed to help employees who smoke to quit or reduce the amount smoked.

More research is needed specifically focussing on the occupational category of blue collar workers. In order to develop content for such interventions, the combination of qualitative and quantitative techniques should be employed, using focus groups, individual interviews and then the strongest possible design to assess effectiveness with regard to a wide variety of outcomes. In addition, the structure of the workplace in terms of occupational groupings, unions, existing policies, norms regarding smoking, national or lower order restrictions on smoking, need to be included so that the content of the evaluation can be specified. This level of information is particularly important when worksites are considering the intervention, programme options that may be relevant, applicable and effective in their particular setting.

Our analysis has involved a search for all available literature on blue collar workers. At the moment, it is not possible to make any specific policy/intervention recommendations based on the available evidence because so few findings have focussed on the group of

blue collar workers.

Our analysis of **School Based Interventions** found that the types of interventions most commonly found in the literature could be classified into two broad types. Interventions that focused on programs, aimed at preventing or delaying smoking initiation, or encouraging cessation in school-aged subjects, formed the bulk of all school based smoking research. Policy based (legislative, or non-legislative) programs, directed at school staff and/or students which mandated specific acceptable or unacceptable smoking related behaviours while in or on school property comprised a much smaller, although significant proportion of the literature.

Based on the above discussion of evaluation methodological issues in school-based smoking programs, several issues of relevance to the evaluation of TDRS activities specific to school-based interventions and school-aged youth emerge. These include:

Valid standardized measures of smoking behaviour for school-aged youth need to be developed and assessed. It is important that such measures be able to identify youth at various stages of the smoking onset process. Definitions used for adolescents and adults (eg. daily smoking) are not likely to be useful for the majority of elementary school-aged smokers, making it necessary to develop instruments which may be age specific. Standardized, rigorous protocols for gathering data in schools need to be developed and implemented in both the context of survey and evaluation research. Issues such as the collection of biochemical data to promote truthful reporting, allowing for the collection of data from students who are absent from initial testing sessions, use of trained data collectors from outside the school, and, in the case of longitudinal designs, tracking of students who move to minimize the effects of attrition, need to be carefully considered for their effects on reported rates and trends.

There is considerable change in smoking onset by single years of age, and this may well vary by gender in school-aged youth. Consequently, data collection in youth needs to be complete enough so that reliable estimates of smoking prevalence, appropriately defined for the age of the child, can be obtained and reported by single year of age and gender. Alternatively, key time points in the smoking onset process need to be assessed with samples large enough to make precise statements about appropriate rates. For example, the transition to secondary school is one such point, and estimates for before and after this transition are especially important in evaluating efforts to prevent smoking onset. Careful attention must be paid to unit of randomization, unit of intervention and unit of analysis issues. As described above, evaluation research in schools almost always involves either the (random) assignment of schools or classes to condition, or the delivery of interventions to groups, or both. In either case, it is reasonable to expect, and experience confirms, that children within a school/class/group will behave more similarly than will students in another school/class/group either because the students interact (and they may be encouraged to do so), or because factors at the group level (eg. SES or provider of the intervention) influence smoking rates for all students in the group similarly. Evaluation designs and subsequent analyses must recognize this phenomena to achieve adequate power or avoid declaring differences significant when they are not.

Careful attention needs to be given to the collection of process data, implementation data, and data on other explanatory variables. Evaluations can achieve greater power with careful attention to blocking (stratification) on variables related to outcome. Analyses can be more efficient and results more informative by carefully considering

subpopulations of youth, schools, providers, etc. where interventions may achieve greater (or lesser) success. Indeed, if it were possible to target interventions more effectively, the overall results of TDRS initiatives could be enhanced. In the design of programs and their evaluation, dissemination issues must be considered. This could be achieved by involving those responsible for the eventual implementation and/or dissemination from the outset. Barriers to the effective implementation of school-based programs have been identified in a survey sponsored by Health Canada and the Canadian Cancer Society. There is need for further research to determine ways to put in place and sustain programs of proven effectiveness.

Evaluating media campaigns summarizes some general methodological issues that tend to be generic to the evaluation of media campaigns. Subsequent sections review three distinct types of media campaigns/ evaluation strategies that are designed to effect very different outcomes: 1) mass media campaigns directed towards encouraging smoking *cessation*; 2) campaigns which focus upon smoking *prevention*; and 3) campaigns undertaken as part of a broad, comprehensive *multi-component* (eg, ASSIST) or *multi-issue* (eg, Heart Health/Healthy Lifestyles) intervention.

The Chapter examines a series of evaluation design and measurement issues, and examples of campaigns in cessation, prevention and campaigns as elements of larger initiatives. Many of the large-scale demonstration projects identified in the chapter provide excellent examples of evaluation designs of relevance to the Tobacco Demand Reduction Strategy.

A general conclusion can be reached in relation to the evaluation of media campaigns. Taken together, the results of our review suggest the importance of integrating media campaign evaluations within the larger context of the macro evaluation framework for the Tobacco Demand Reduction Strategy.

Our analysis of **Tobacco prices and their Effect on the Use of Tobacco** assesses methodologies used to evaluate the effectiveness of tobacco prices and taxation as an intervention made by public authorities in countries other than Canada to reduce the use of tobacco.

Price changes of goods and services have been known to affect the individual and collective health consumption and production decisions. Prices have been used, both intentionally and inadvertently, to affect health behaviours. Pricing and taxation of tobacco and other products, such as alcohol, have long been used as means of generating revenue for governments. Historically, cigarette excise taxes have not been levied primarily to discourage smoking, rather, these taxes have been viewed as a convenient and effective revenue-raising device. Nevertheless, there is abundant evidence that cigarette price has a significant impact on consumption.

Basic concepts of demand, supply, price and elasticity are reviewed in this chapter. The methodologies (model types) and data types for estimating price elasticity are described and compared. A comprehensive review of the use of various methodologies (model types) and data types to evaluate the price elasticity for the demand for tobacco products in various countries other than Canada is given. Lastly, the effectiveness of using price elasticity, and a number of weaknesses and difficulties in the estimation procedures are pointed out, and a number of ways for improvement are suggested.

Furthermore, results shown in Table 1 suggest the tremendous potential of cigarette excise taxes as an incentive to improve health behaviour and to cut down on the number of smokers. We have also identified a number of limitations to using price elasticity to estimate the effectiveness of pricing and taxation in the reduction of tobacco use. We have also identified problems and difficulties in using a variety of methodologies for the estimation of elasticity in defining. Additional problems may be encountered in defining exploratory variables. Finally problems in estimating elasticity may also be found due to the choice of model formulation.

This analysis is offered as an overview of elements to consider in estimating the impact of price increases on tobacco use.

As a result of a review of available literature, the most prevalent interventions and readily accessible evaluation information for Non-Price Restrictions relate to four smoking intervention programs; Smoking Restrictions in Public Places, Reducing Youth Access to Tobacco, Advertising, and cigarette warnings.

Our analysis of a wide variety of options available for the use of non-price restrictions has identified four areas most commonly utilized and evaluated in the literature.

Smoking Restrictions in public places were evaluated by outcome evaluations utilizing pre and post smoking restriction measures. The lack of continual surveillance and no control community make it difficult to attribute behaviour changes directly to smoking restrictions.

Laws restricting youth access to tobacco have most often been evaluated by repeat cross sectional observations or surveys at several points in time. The studies are most often formative and outcome evaluations.

The effects of advertising restrictions on smoking are difficult to measure. Studies have not been able to attribute any increase in smoking initiation rates solely to an increase in advertising expenditures.

Cigarette warnings are the fourth intervention area identified in our review. Despite the enormity of this focussed effort to prevent smoking, there is little or no evidence that the warnings are effective. At this point the majority of studies have examined potential effectiveness of warnings due to the measurement difficulties for behavioral effectiveness.

Finally a series of **recommendations are offered for use in evaluating the federal TDRS program.**

1. That the TDRS establish an external scientific review/advisory committee of researchers which represent expertise in a full range of methodologies specializing in evaluation for Intervention areas identified in this report to advise on all aspects of TDRS monitoring, tracking and evaluation.

2. That evaluation assessment studies be carried out on all intervention activities undertaken under TDRS, and that specific evaluation frameworks be developed for these components. The evaluation methodologies selected for each intervention should reflect the need and possibility for rigour and precision suitable to each specific situation.
3. That all evaluation strategies relating to the components of TDRS be subject to external review, or at a minimum, subject to review by an evaluation advisory committee.
4. That specific attention be accorded the major threats to external and internal validity (described in this report) that typically are encountered in the evaluation of national intervention programs.
5. That specific attention be accorded the measurement issues of greatest concern to Tobacco Control evaluations.
6. That a multiple-methods research strategy be developed that "triangulates" results relating to the components of TDRS.
7. That specific measurement issues for each of the intervention areas (eg. media, community, school etc.) of exposure to all TDRS components be incorporated into tracking and evaluation studies.
8. That Health Canada consider commissioning additional studies on the outcomes of evaluations. These studies could utilize materials derived from this study and a similar report completed on Canadian evaluation approaches.

TABLE OF CONTENTS

1	Introduction	11
1.1	Investigator TEAM	11
1.2	Project Approach	12
1.3	Literature Data Bases Searched	12
1.4	National and International Contacts	13
1.5	Multi Component Intervention Background Information	14
1.6	Report Content and Approach	17
1.7	References - Additional Information	18
2	Methodological Overview	19
2.1	Types of Evaluation	19
2.2	Overview of Evaluation Methodologies	20
2.3	Methodological Issues in Evaluation of Interventions	21
2.4	References	25
3	Specific Interventions	27
3.1	Review Of Community Based Initiatives	27
3.1.1	Introduction	27
3.1.2	General Strategies	29
3.1.3	Studies - Outcome Measures	30
3.1.4	Conclusions	37
3.1.5	References - Additional Information	39
3.2	Worksite Based Initiatives For Blue Collar Workers	45
3.2.1	Introduction	45
3.2.2	General Strategies	48
3.2.3	Studies - Outcome Measures	49
3.2.4	Conclusions	59
3.2.5	References	61
3.3	School Based Smoking Interventions In Countries Other Than Canada	70
3.3.1	Introduction	70
3.3.2	General Strategies	70
3.3.3	Studies/Outcome Measures	73
3.3.4	Conclusions	77
3.3.5	References - Additional Information	80
3.4	Evaluating Media Campaigns	83
3.4.1	Introduction	83
3.4.2	General Strategies	84
3.4.3	Studies/Outcome Measures	87
3.4.4	Conclusions	93
3.4.5	References - Additional Information	95

3.5	Tobacco Prices And Their Effect On The Use Of Tobacco	100
3.5.1	Introduction	100
3.5.2	General Strategies	101
3.5.3	Studies - Outcome Measures	105
3.5.4	Conclusions	113
3.5.5	References/Additional Information	114
3.6	Non-Price Restrictions	120
3.6.1	Introduction	120
3.6.2	General Strategies	121
3.6.3	Studies - Outcome Measures	123
3.6.4	Conclusions	127
3.6.5	References	129
APPENDIX A		130
APPENDIX B		133
APPENDIX C		145
APPENDIX D		148

1 Introduction

The Program Evaluation Division of Health Canada has commissioned a critical review of evaluation methodologies for tobacco control interventions in other countries. The review was designed to assist the Program Evaluation Division in preparing the evaluation of the Tobacco Demand Reduction Strategy (TDRS). This report is to be used as a basis for selection of optimum approaches.

This report offers an analysis of Tobacco Control Program interventions with an emphasis on evaluation methodologies used in other countries. The analysis focusses on a critical assessment of methodological approaches used to study the effectiveness of the interventions. The report does not focus on the analysis and evaluation of the effectiveness of the interventions or their outcomes.

The level of analysis and approach to the review of intervention areas varies by the amount of available literature and information available for review, the complexity and scope of the intervention area and the focus of the analysis. The project team has focussed on evaluation methodology issues as stated by the contract terms of reference. A detailed analysis of intervention programs and their outcomes awaits future study within a new mandate.

1.1 Investigator TEAM

The team of investigators assembled to complete project tasks and activities are the Principal investigators involved with the Ontario Tobacco Research Unit (OTRU). The Unit was established by the Ontario provincial Ministry of Health in 1993 to: conduct regular reviews of the literature in priority areas, monitor the progress of the strategy, participate in research projects for new learning, and is the Ontario linkage to the international network of scientists committed to tobacco control.

The investigators are internationally recognized experts in the field of tobacco and program evaluation. Furthermore, OTRU has an extensive network of collaborating investigators and international contacts. We have drawn on these networks in the completion of this report.

Our team has completed this report on the basis of the following assignments:

- Preparation of Methodological Template/Overview -Dr. Stephen Brown
- Areas of Intervention:
 - Workplace Interventions* - Dr. Linda Pedersen, Dr. Mary Jane Ashley
 - Community Interventions* - Dr. Elizabeth Lindsay
 - School Programs* - Dr. Stephen Brown
 - Media* - Dr. Irving Rootman/Mr. Reg Warren
 - Pricing and Taxation* - Dr. Roberta Ferrence, Dr. Bernard Choi
 - Non-Pricing Interventions* - Dr. Tom Abernathy
- Project Coordination- Mr. Rick Wilson

N.B Further information concerning the experience and qualifications of the Investigator team are attached. (Appendix C)

1.2 Project Approach

The project team adopted the following approach in completing the project and this report:

- Initial project team planning meeting on awarding of contract.
- Development and transmittal of contact form letter to international contacts to obtain unpublished information (see section 1.4).
- Extensive literature data base searches and literature review.
- Regular project team progress/coordination meetings.
- Development of report outline and format guidelines.
- Progress report and meeting with Health Canada.
- Preparation and joint review of report content.

The following sections of the introduction offer further information on specific elements of this approach.

1.3 Literature Data Bases Searched

The completion of our analysis involved the collection and comprehensive review of information from the following data bases:

1. Mass Media

- Smoking and Health
- Wilson Index
- Medline
- Health Promotion and Education
- Psychology Literature (PSYCLIT)

2. Community

- Smoking and Health
- Medline
- Health Promotion and Education
- Psychology Literature

3. Workplace

- Smoking and Health
- Wilson Index
- Medline
- Health Promotion and Education
- Psychology Literature

4. Pricing/Taxation
 - Smoking and Health
 - C.B.C.A.
 - Wilson Index
 - ABI/Inform
5. Non-Pricing Interventions/Legislation
 - Smoking and Health
 - Wilson Index
 - ABI/Inform
 - C.B.C.A.
6. School Based
 - Smoking and Health
 - E.R.I.C.
 - Comprehensive School Health

We have included extensive references to sources and additional information as appropriate within each section of the report.

1.4 National and International Contacts

As a further effort to ensure a comprehensive information collection process we have contacted and attempted to obtain supplementary and/or unpublished information. These individuals contacted include:

- COMMIT (U.S. Can.) - All COMMIT Investigators (10)
- STANFORD - June Flora, D. Altman - *Principal Investigators*
- NORTH - Karelia P. Puska - *Principal Investigators*
- MINNESOTA - M. Middlemark, D. Murray C.Perry, Pirie - *Co Investigators*
- PAWTUCKET - T. Lasater - *Principal Investigators*
- MASSACHUSETTS - Ockene, G. Sorensen - *Principal Investigators*
- CALIFORNIA - J. Moscovitz, J. Pierce, C. Edwards - *Co Investigators*
- VICTORIA, AUSTRALIA - D. Hill - *Principal Investigator*
- WASHINGTON - A. PetersOn - *Principal Investigator*
- CHICAGO - B. Flay - *Investigator*

Our response rate with regards to additional/unpublished material has been minimal and is an indication that our extensive literature searches have provided the majority of available information.

1.5 Multi Component Intervention Background Information

The extensive literature review conducted for this project has identified hundreds of references to studies and intervention. Individual chapters of the report offer explanatory notes for references to a level sufficient for understanding of the analysis of evaluation methodologies. In order to further assist the reader, we offer the following background information for major multi component Tobacco Control Interventions:

- **COMMIT** - The Community Intervention Trial for Smoking Cessation was a 4

year 1989-1993) community-level effort aimed at increasing quit rates among smokers, with a particular emphasis on hard to reach heavy smokers. The trial involved 11 matched pairs of treatment and comparison communities throughout the United States and Canada (1 matched pair Brantford and Peterborough). Activities in the randomly selected intervention communities focussed on four primary channels: public education through media and events, health care; worksites and religious organization; and cessation resources. The main hypothesis tested in the trial was that heavy smokers exposed to these activities delivered through multiple channels would achieve a 10 percent higher quit rate than unexposed heavy smokers in comparison communities. The main results of the trial appear in the February 1995 issue of the American Journal of Public Health. The 4 year intervention impacted significantly on light to moderate smokers but did not produce a statistically significant difference in the cessation rates among heavy smokers.

- **ASSIST** - America Stop Smoking Intervention Study, a major initiative under way in 17 States is funded by the National Cancer Institute (NCI). It is designed to help 4.5 million adult smokers to quit and to prevent 2 million youth from initiating smoking. ASSIST is a major collaborative intervention designed to use the latest and most effective tobacco control methods to reduce tobacco use. In addition to reducing the numbers of people addicted to tobacco, ASSIST also concentrates on widespread policy changes and media advocacy designed to promote non use of tobacco as the social norm.

Policy objectives are: to mandate clear indoor air; to restrict access to tobacco by minors; increase economic incentives to discourage the use of tobacco products and to restrict the advertising and promotion of tobacco.

- **Stanford Five-City Project**

The project was initiated in 1978 to evaluate the effects of a community-wide health education program on cardiovascular risk factors, including blood pressure. Two treatment cities received an education program which used the mass media, various community-based programs and health professionals designed to encourage individuals to learn their blood pressure levels, exercise regularly and reduce dietary sodium. Additional studies were conducted to determine the percentage of smokers reporting that a physician had ever advised them to smoke less or stop smoking and the effect of time, demographics, medical history and cigarette dependence on the likelihood that respondents would state that a physician had ever advised them to stop smoking. Finally, a variety of studies were conducted on smoking prevalence, trends in adolescents and young adults.

- **North Karelia Heart Health Project**

The North Karelia project was started in 1972 as a response to the request of the local people. It was aimed at reducing the exceptionally high mortality and morbidity from cardiovascular diseases in the area. The Project's activities have been directed towards inducing behavioural changes against the entire population. The strategy of community based prevention was based on the previous pioneering international work on epidemiology of cardiovascular diseases and prevention in which Finland had in many ways participated. Comprehensive

scientific evaluation of the programme was carefully designed. The effects of the programme on risk factor reduction and on prevention of cardiovascular diseases are assessed by large population surveys every five years and by data on incidence of cardiovascular diseases and mortality rates. A series of studies have been conducted assessing the impact and outcomes of the project.

- **The Minnesota Heart Health Program**

The Minnesota Heart Health Program is a 13 year research and demonstration project to reduce morbidity and mortality from coronary heart disease in whole communities in the Upper Midwest. The program was designed to promote more frequent and vigorous physical activity in North American communities along with improved eating and smoking patterns. The program is ongoing in three pairs of communities in Minnesota, North and South Dakota. The strengthened inference of program effects, its basic design involves elements of control, repetition, sensitive trend measurements and evaluation of the effects of program components. The design is able to compare risk factor levels and mortality rates between education and comparison communities. The statistical power is sufficient to detect community-wide changes of public health import.

- **The Pawtucket Heart Health Project**

The Pawtucket Heart Health Program was a research effort testing a process of community activation for cardiovascular risk factor behavior change and coronary heart disease event rate change, utilizes risk factor behavior change programs for the entire population of a northeastern city. The program is designed to effect a community-wide change in heart-disease morbidity and mortality by reducing smoking prevalence and other behavioral risks for this disease. Evaluation methods included biennial household risk factor surveys in the education and comparison communities, morbidity and mortality surveillance in hospitals serving each community, and both formative evaluation and process tracking. The intervention strategy began as an organization-based model and has evolved into a community-wide effort in which marketing research tools and techniques are employed.

- **Massachusetts Heart Health Program**

A statewide program launched by the Department of Public Health to prevent deaths from heart disease, cancer and cardiovascular diseases by reducing their underlying risk factors. The goals are to eliminate or reduce smoking, unhealthy diet, high blood pressure, physical inactivity, and environmental toxic exposure as risk factors. The program utilizes a multi-component approach that includes risk prevention programs, smoking cessation, blood pressure control, heart disease, cancer and stroke risk intervention programs and policies and anti-smoking public education campaigns.

- **Toward a Tobacco-Free California - California Tobacco Control Program**

The program uses multiple strategies for discouraging young people from initiating tobacco use and for providing encouragement and support to enable current tobacco users to quit. The program targets specific populations known to

be a highest risk for tobacco related disease and death. It has been established (1) in all 61 of the State's local health departments (2) in 58 county offices of education, 10 Healthy Kids Regional Centres and over 1,000 public school districts and (3) in over 500 individual community based projects across the State.

- **New South Wales**

North Coast Healthy Lifestyles Program involved a multi-component approach in Australia. The focus was on media and Community Activities and included the "Quit for Life" anti-smoking campaign, the "Give it a Go" motivational program, the "Get it Off" anti-obesity/exercise campaign and the "Be Nice to Your Body" general health promotion. The primary aims were to reduce smoking, increase exercise and lower saturated fat intake in the adult population. Secondary objectives were to reduce incidence of obesity, hypertension and accidents and to exercise stress control and self-responsibility in health care.

1.6 Report Content and Approach

Our analysis comprises the following:

- Methodological Overview
 - Types of Evaluation
 - Types of Evaluation Methodologies
 - Methodological Issues
- Areas of Intervention

These areas for intervention include: Workplace, Community Interventions, School Programs, Media, Pricing and Taxation, Non-Pricing Interventions.

- An introduction that will briefly describe the types of intervention approaches.
- General Strategies employed for evaluating the type of intervention.
- Exemplar studies description - Outcome measures employed.

Each section will contain appropriate references identified in the Chapter in a format appropriate to the preferences of the Chapter author.

Each section should also contain an appendix for all projects/literature considered in preparing the chapter (i.e. for further information).

1.7 References - Additional Information

1. Lindsay, E. Receipt of Intervention Proposal, N.H.R.D.P., April 1995.
2. Minnesota Department of Health, Section for Non-Smoking and Health; American Cancer Society, Minnesota Division. American Stop Smoking Intervention Study: ASSIST Minnesota Site Analysis.
3. Fortman, SP, Winkleby, MA, Flora, JA, Haskell, WL, Taylor, CB. Effect Of Long-Term Community Health Education On Blood Pressure and Hypertension Control. The Stanford Five City Project. American Journal of Epidemiology, 1990, 132 (4), 629-46.
4. Puska, P, Korhonen, HJ, Torppa, J, Tuomilehetto, J, Vartainen, E, Pietinen, P, Nissinen, A. Does Community-Wide Prevention of Cardiovascular Diseases Influence Cancer Mortality. European Journal of Cancer Prevention, 1993, 2 (6), 457-60.
5. Jacobs, Dr, Jr, Luepker, RV, Mittlemark, MB, Folsom, AR, Pirie, PL, Mosuoli, SR, Hannan, PJ, Pechasek, TF, Bracht, NF, Carlaw, RW, et al. Community-Wide Prevention Strategies: Evaluation Design of the Minnesota Heart Health Program. Journal of Chronic Diseases, 1986, 39 (10), 775-88.
6. Carleton, RA, Sennet, L, Gams, KM, Levins, S, Lefebvres, L, Kassater, TM. The Pawtucket Heart Health Program Influencing Adult Eating Patterns. Annals of the New York Academy of Sciences, 1991, 623: 322-6.
7. Havas, S, Walker, B. Massachusetts Approach to the Prevention of Heart Disease, Cancer and Stroke. Public Health Reports, 1986, 101 (1), 29-39.
8. California Department of Health Services, California Department of Education Toward a Tobacco-Free California: a Status Report to the California Legislation on the First Fifteen Months of California's Tobacco Control Program.
9. Egger, G, Fitzgerald, W, Frape, G, Monam, A, Rubinstein, P, Tyler, C, McKay. Results of Large Scale Media Anti-Smoking Campaign in Australia, North Coast Quit for Life Programme.

2 Methodological Overview

2.1 Types of Evaluation

In a **needs assessment**, the extent of the problem being addressed is determined, as well as the need for the program. Program providers attempt to determine whether needs are already being met by other similar programs, or whether there is a gap in services that their program could resolve. Other questions that a needs assessment can answer, include whether the intended target audience is aware of other existing programs, or whether they even desire the service. "Periodic" needs assessments are conducted after program implementation to determine whether there is still a need for the program, and/or whether there are other potential target audiences that could be reached. Needs assessments often utilize key informant interviews, focus groups, community surveys or epidemiologic data.

An **implementation evaluation** involves monitoring the implementation of a new program to ensure that the program is being delivered as planned, usually by comparing implementation performance to some sort of predefined standard measures. This type of investigation can also determine whether the program is actually reaching the intended target audience, and whether there are any barriers that prevent the target audience from participating in the program. Data sources include client and program records, and possibly community surveys.

A **process evaluation** is similar to an implementation evaluation, except the program of interest has already been implemented. Again, the program is monitored to see that it is being implemented as planned (implementation standards may have fluctuated over time), whether the intended target audience is being reached, and to identify any barriers to program use.

An **outcome or impact evaluation** should be undertaken either concurrently with or after an implementation or process evaluation that has demonstrated that the program is being delivered as intended, in order to avoid confounding deficiencies in program implementation with lack of program impact. An outcome evaluation investigates whether the program is having the desired effect, based on a clearly defined set of client objectives, and should effectively rule out any alternative explanations to client outcomes. Data sources include measurements of the targeted client behavioural characteristics at least at the end of the program, and often before the program and/or during some follow-up period.

A **comprehensive evaluation** is simply an evaluation in which a needs assessment, process or implementation evaluation, and an outcome evaluation are all conducted.

A **formative evaluation** refers to the research, development and pilot-testing of a new program. A formative evaluation could include activities ranging from a needs assessment, focus groups or interviews to assess audience response to the proposed program, pilot-testing of the program, and/or a mini outcome evaluation.

A **cost-benefit evaluation** determines whether the cost per unit outcome achieved

is acceptable, while a **cost-effectiveness evaluation** can be used to compare programs to determine the least-cost method of achieving the desired outcome.

2.2 Overview of Evaluation Methodologies

Any review of program or policy interventions can expect to encounter a wide variety of study designs. These can range from passive or observational studies in which data are collected and/or analyzed with the investigator exercising little control over sample selection, through sample survey research in which the investigator chooses a sample from a target population using a well defined sampling scheme, to designs involving formal random assignment of units to either intervention or comparison conditions.

While the clinical trial model, involving randomization of subjects or groups of subjects (e.g. worksites, schools, communities) to condition is the strongest in terms of allowing cause and effect conclusions, the type of study design will depend on issues such as the degree to which randomization to condition is feasible, the available resources, and the stage in the evolution of the intervention.

For example, the study of the effect of price on consumption will normally rely on the analysis of available time series data, and the degree to which other variables can be controlled will depend on the amount and quality of data on these variables, or on the existence of similar data from other jurisdictions.

The effect of public policy changes, worksite, school or community interventions on attitudes or behaviour may be assessed by repeat cross-sectional surveys in study populations at several points in time (e.g. before and after the change/intervention). Alternatively, a before-after cohort design in which the same individuals are assessed at two or more points in time may be considered. Showing a different pattern of change in comparison or control communities greatly strengthens the conclusions should either of these before-after comparisons prove significant. Finally, the formal randomization of units to intervention or comparison conditions provides the strongest test of an intervention, but increases study complexity.

In any case, in examining the evaluation of an intervention, close attention needs to be paid to issues such as the choice of the target population and the method for obtaining the study population from that target population; the degree of randomization and replication incorporated in the study design; the choice of outcome variable(s); the presence of other (independent) variables; the reliability of instruments and procedures used to collect the data; data completeness; the methods (both design and statistical) used to adjust for possible confounds; and the choice of data analysis technique given the type of data available on the relevant variables, the study design, and the model linking the intervention to the outcome variable(s).

2.3 Methodological Issues in Evaluation of Interventions

In this section we list issues common to almost all evaluations of smoking prevention, cessation or protection interventions. There are a number of excellent reviews of methodological concerns in the evaluation of interventions. Some of these have been referenced beside the particular concern, and the reader can

consult these for more detail.

We will discuss these issues in an order designed to reflect the basic stages of the evaluation from the conceptualization of the intervention, through the study design and measurement of outcomes, to the analysis of data and presentation of conclusions.

Questions to ask of an evaluation report.

1. What is the intervention?⁽¹⁻³⁾
Can it be clearly defined?
What is the unit of intervention? i.e. How is it delivered - to individuals, groups, communities, etc?
2. What is the theory linking the intervention to the behaviour (change)?⁽²⁻⁴⁾
What would this theory predict concerning intermediate endpoints, mediating variables, etc.?
3. What is the nature of the trial or study in which the evaluation is conducted?⁽⁵⁾
Is it an efficacy trial, an effectiveness trial, an implementation effectiveness trial, a demonstration trial, etc.?
4. What is the target group for the intervention?^(2,3)
Who is the intervention attempting to reach?
What is the mechanism for reaching this target population?
5. What is the study population?
What are the social units/individuals who could have participated in the evaluation?
6. What is the sampling mechanism?
How did the social units/individuals who participated get chosen?
7. What are the outcome measures?^(2,5,6,7)
From whom are they collected - providers of the intervention, participants, social units, etc.?
What are the choices? Process measures such as implementation fidelity, provider and setting characteristics, etc; Impact measures include intermediate endpoints such as knowledge, beliefs, attitude, etc.; outcome measures such as amount smoked, or prevalence; theoretical constructs such as stage of change; economic indicators such as price; legal measures such as infractions reported; measures of health such as morbidity or mortality.
Are measures short term (immediate post intervention) or longer term (12 months post intervention)?
8. How frequently are measures taken?⁽²⁾
9. What other data were collected?⁽²⁾
How are they used?
10. What is the reliability and validity of the measurement system (i.e. instruments and measurement procedures?)⁽⁸⁾

What are conditions under which data are collected?

Are there standard protocols? Who collects data? Are data collectors trained?

What collection method(s) is (are) employed? Archival searching, secondary analysis of existing data, telephone surveys, questionnaire data, direct observation, focus groups, structured or open-ended interviews.

What is used to promote truthful response? Confidentiality assured, biochemical validation?

11. What is the design?^(4,5,9-13)

What is the effect size?

What is the study type?

Observational - data collected without investigator control over assignment to groups (e.g. archival data, case-control studies, "quasi-experimental" designs).

Sample survey data - including repeat cross-sectional surveys of the same study population.

Longitudinal follow-up of defined cohorts.

What is the control/comparison group?

(How) Was randomization employed?

Survey data - what is the sampling frame, sampling method (e.g. simple random sampling, cluster sampling, etc.).

Longitudinal data - what is the unit of randomization? (Individuals, intact social units/clusters).

What is the study size? (Replication)

How many units were to be (randomly) selected/assigned?

How many providers were to be involved?

How many study participants/social units were to be involved?

Was there blocking/stratification prior to randomization/sampling?

Which variables were blocking/stratification variables? Why were they chosen?

What is the power of the design?

12. What is the study attrition rate? Non-response rate?^(4, 14)

How many units were actually selected/assigned?

How many units, providers, participants, etc. were involved at each stage?

How many units, providers, participants, etc. provided data at each stage?

What rates are reported?

Per cent of eligible units/participants, per cent with consent?

Is attrition (likely) related to outcome?

13. How were the data managed, linked, etc.?

14. What is done to assess comparability of intervention and control groups at baseline?⁽¹⁵⁾

Which measures are assessed (demographics, behavioural, etc.)?

How is this analysis done? (see below).

15. What is done to assess differential attrition between groups?

Which measures are assessed?

How is this done?

16. What methods of statistical analysis are used?^(6,7,10,15)
Are the methods appropriate given any special properties of the measures (e.g. distributional properties)?
Do methods take into account intracluster correlation when the unit of intervention and/or the unit of randomization is not the individual?
How do the methods take into account the incomplete nature of the data (missing data, loss to follow-up, etc.)?
How are design variables built into the models used for analysis?
What adjustments are made for possible confounding variables?
17. Does the analysis link theory to process to outcome?^(5,7)
18. How are results reported?⁽⁴⁾
Is there a point estimate of effect size?
Is there a measure of the variability of the point estimate?
What can be said about relevant subgroups of the study population?
19. Are the conclusions justified in light of the discussion above?⁽⁴⁾
Does the intervention work?
What has been learned about the theory - process - outcome relationship?

2.4 References

1. Jackson, C, Altman, DG, Howard-Pitney, B, Farquhar, JW. Evaluating Community-Level Health Promotion and Disease Prevention Interventions. In Evaluating Health Promotion Programs. M. T. Braverman (ed). New Directions for Program Evaluation, 1989, 43, 19-32.
2. Collins, LM. Some Design, Measurement, and Analysis Pitfalls in Drug Abuse Prevention Research and How to Avoid Them: Let Your Model Be Your Guide. Scientific Methods for Prevention Intervention Research, NIDA Research Monograph Series #139, 1994, 95-114.
3. Flay, BR, Petraitis, J. Methodological Issues in Drug Use Prevention Research: Theoretical Foundations. Drug Abuse Prevention Intervention Research: Methodological Issues, NIDA Monograph #10, 1991, 81-99.
4. Moskowitz, JM. Preliminary Guidelines for Reporting Outcome Evaluation Studies of Health Promotion and Disease Prevention Programs. In Evaluating Health Promotion Programs. M. T. Braverman (ed). New Directions for Program Evaluation, 1989, 43, 101-112.
5. Flay, BR. Efficacy and Effectiveness Trials (and Other Phases of Research) in the Development of Health Promotion Programs. Preventive Medicine, 1986, 15, 451-474.
6. Best, JA, Brown, KS, Cameron, R, Smith, EA, MacDonald, M. Conceptualizing Outcomes for Health Promotion Programs. In Evaluating Health Promotion Programs. M. T. Braverman (ed). New Directions for Program Evaluation, 1989, 43, 33-45.
7. MacKinnon, DP. Analysis of Mediating Variables in Prevention and Intervention Research. Scientific Methods for Prevention Intervention Research, NIDA Research Monograph Series #139, 1994, 127-154.
8. D'Onofrio, CN. The Use of Self-Reports on Sensitive Behaviours in Health Program Evaluation. In Evaluating Health Promotion Programs. M. T. Braverman (ed). New Directions for Program Evaluation, 1989, 43, 59-74.
9. McKillip, J. Evaluation of Health Promotion Media Campaigns. In Evaluating Health Promotion Programs. M. T. Braverman (ed). New Directions for Program Evaluation, 1989, 43, 89-100.

10. Programs. M. T. Braverman (ed). New Directions for Program Evaluation, 1989, 43, 75-88.
11. Hsieh, FY. Sample Size Formulae for Intervention Studies with the Cluster as Unit of Randomization. *Statistics in Medicine*, 1988, 114, 1195-1201.
12. Hannan, PJ, Murray, DM, Jacobs, DR, McGovern, PG. Parameters to Aid in the Design and Analysis of Community Trials: Intraclass Correlations from the Minnesota Heart Health Program. *Epidemiology*, 1994, 88-95.
13. Donner, A, Birkett, N, Buck, C. Randomization by Cluster: Sample Size Requirements and Analysis. *American Journal of Epidemiology*, 1981, 114, 906-914.
14. Hansen, WB, Collins, LM, Malotte, CK, Johnson, CA, Fielding, JE. Attrition in Prevention Research. *Journal of Behavioural Medicine*, 1985, 8, 261-275.
15. Donner, A, Brown, KS, Brasher, P. A Methodological Review of Non-Therapeutic Intervention Trials Employing Cluster Randomization, 1979-1989. *International Journal of Epidemiology*, 1990, 19, 795-800.

3 Specific Interventions

3.1 Review Of Community Based Initiatives

3.1.1 Introduction

This section of the report addresses evaluations of community-based cessation programs which were directed toward women, low income groups, people with identified smoking related illnesses, as well as the community as a whole.

We will first describe issues which need to be considered in planning an evaluation for community-based initiatives. We will then describe the types of approaches which have been applied at the community level to help smokers stop smoking. Several examples of different types of evaluations will be provided which demonstrate how these issues have been addressed. A bibliography of programs is attached.

Issues To Consider In Designing An Evaluation

Etiology of Smoking

The etiology of smoking is grounded in physical, social, psychological, behavioral and spiritual phenomena. Interventions designed to address smoking cessation will approach individuals and communities at all of these levels. Outcomes may occur within any or all of these. The development of programs and evaluations requires knowledge from many disciplines such as social psychology, communications, medicine as well as knowledge of research design, sampling methods and data analysis, evaluations require familiarity with the structure and dynamics of community systems, such as the educational, commercial, industrial and political systems.⁽¹⁾

Issues: What is the appropriate outcome measure? Are the objectives of a program realistically matched to outcomes?

Process and Content within Programs

Community-based initiatives are multi-faceted. They usually occur across multiple settings, using multiple change strategies, to achieve broad and repeated exposure to a set of complementary and interdependent activities⁽¹⁾. There may be two quite incompatible goals at work. The desire to maximize the probability that the intervention will make a difference which means implementing a comprehensive intervention and secondly they want to know which components did or did not contribute to the effects. The greater the success at implementing a comprehensive set of activities, the more difficult it becomes to disentangle specific parts of the strategy.

Successful approaches include a substantial organizational component as well as attention to the program itself. Therefore the evaluation of these programs is also complex involving several types and stages.

Issues: What are the indicators of successful process?

How do we integrate many data sources? How do we measure change at the community or organizational level? What is the appropriate unit of analysis? Is it best to look at a set of interventions rather than separate components? How do we

know which are the most effective components?

The Target of the Intervention: Individuals or Communities

If the intervention is directed at a community, the impact is measured through surveys of individuals. There are many factors which affect what type of analysis is appropriate. Finding appropriate comparison groups when randomization is not possible are affected by the unique features of communities i.e. social, economic, political issues. These may combine to affect the impact of an intervention and make another community an inappropriate comparison group.

Issues: This has implications for degree of freedom, appropriate comparison groups which have important implications to the interpretation of the findings.

Understanding Change

One needs to understand the process of change within the health issue one is studying. A thorough study of helping people change will reveal that behavior change is a long term process involving knowledge, attitudes, behaviors which are precursors to the actual change under study. Change also involves many efforts and many steps backwards before the change becomes an integral part of a person's way of life. For example, one rule of thumb suggested by behavior change experts is that there is an average of seven years between a person's first serious effort to stop smoking and the final successful attempt which results in being an ex-smoker. These steps on the way to change are important to understand and measure in evaluation of behavior change.

Issues: What is the proper endpoint at particular times in a process? How much change in what variable is a measure of success?

The Importance of Timing

At all stages of program evaluation, it is important to match the timing of the evaluation with sensible expectations regarding what is being measured. The early stages of community analysis and planning will be driven by the pace at which the community typically considers problems and takes action. This will vary and two years of preliminary work before the start of community-wide education may be needed ⁽²⁾. Changes in CVD morbidity and mortality are likely to take at least 10 -15 years depending upon the unit of analysis. To detect differences in risk behaviors for a community is likely to take at least 5 years depending upon the intensity of the intervention. Changes in knowledge, attitudes, or behaviours might occur within a short time span in a specific group of people, if a program is designed specifically for the needs of the target group and is at an intensity appropriate to the changes desired.

Issues: What is an appropriate outcome measure for a given intervention and when should it be measured?

Who is in control of the program?

Community-based programs have an underlying philosophy regarding a high degree of local control over the purpose and scope of the intervention and therefore the evaluation as well. Community priorities may put different emphasis on evaluation purposes than a research institution driven evaluation.

Issues: How much control can one give up and still maintain sufficient rigour to answer the evaluation question? Who will fund a project which has a flexible design? Who will publish the results?

Restrictions imposed by Ethical issues

The need for control groups is limited by a principle of universal access to services at the community level.

Issue: Control groups are rarely possible in community settings.

3.1.2 General Strategies

Types of Interventions

The nature of the evaluations is of course driven by the kinds of interventions which need to be evaluated. In this discussion, we include comprehensive strategies which combine many different initiatives over several years as well as specific time limited programs usually targeted at smokers in general. When possible, we will refer to programs which attempted to help the special groups of people and the issues that arise with these special groups.

Comprehensive Combinations

It is well accepted that to reach a whole community of smokers requires activities which serve many purposes including those that are educational, those directed toward changing policy and those which promote environmental changes. In these projects, the unit of analysis is sometimes communities and sometimes the

smokers within communities. The CVD risk reduction trials and the COMMIT are examples of these comprehensive studies.

Specific Programs

Contests and competitions have been the most frequently implemented initiatives at the community level. These efforts attract attention to the issue and help people make a quit attempt. Several examples of these programs are included below.

Self-help Programs are low intensity, easy to deliver and inexpensive. They often take the form of print materials or other media such as TV programs.

Group Cessation Programs are usually a part of community-based initiatives. They serve a very small segment of the population but have demonstrated important success for those smokers who attend.

3.1.3 Studies - Outcome Measures

Evaluation Designs And Examples

Study designs will reflect a large number of considerations. One of the most important should be the question which needs to be answered. In the following section, we will demonstrate the designs used to answer certain kinds of questions and provide brief descriptions of studies which exemplify each type.

Evaluation question 1.

What do we know about the setting, characteristics of the client group, past experience which will maximize the impact of our intervention?

Shea et al, 1993

Stephen Shea's⁽³⁾ description of the Washington Heights-Inwood Health Heart Program is what is called a "third generation community-based project", demonstrates the implementation of what has been learned in the quasi-experimental studies in less controlled community situations. This program is one of eight communities funded to do CVD programs and this one is directed at a population of approximately 200,000 people, predominantly Hispanic and of low socioeconomic status living in northern Manhattan in New York City. The focus of the evaluation effort in this study is on community analysis, formative evaluations and careful monitoring of activities in order to direct scarce resources in the most effective directions. Risk factor surveys are conducted at the State level with higher sampling density in eight program communities and at lower density throughout the rest of the state. The survey which is based on self-report of risk factors, will be repeated after 5 years. The intention is to compare the eight funded communities with the other parts of the state. This project is an example of a new emphasis on spending resources on evaluation processes which will improve a program or indicate whether a program has reached the people it was intended to reach and whether it had an impact on those individuals. This is characteristic of "diffusion research" in which questions are directed at how a previously tested intervention can be adapted successfully in a different situation.

Implications

It is often difficult to find funding for this type of evaluation. Often, these type of

data exist but are not put together in an accessible place or format for programmers to access.

Evaluation question 2.

Does the intervention have face validity? Has it been previously tested and found to work? Do the modifications for this setting make sense? Will the intervention reach the target audience and are they affected by it? (Formative Evaluation, Process Evaluation)

COMMIT, 1990

The Community Intervention Trial for Smoking Cessation (COMMIT)⁽⁴⁻⁷⁾ was a randomized controlled trial involving eleven pairs of communities. The intervention protocol included 48 activities directly at heavy smokers as the primary target audience. There was extensive community analysis, baseline surveys and extensive monitoring of activities through computerized program records as well as qualitative descriptions of how interventions were carried out in the eleven communities (Lichtenstein, Mattson, Corbett, the COMMIT research group).

This study provides an array of methodologies for different evaluation purposes. The community analysis described existing resources, sectors of influence, leaders and history of collaborative and public health efforts. In addition, force field analysis delineated facilitating and inhibiting factors in the community (Corbett)⁽⁶⁾. The baseline surveys provided important information which assisted with program implementation. The process evaluation methods assessed whether programs met their objectives, how programs were implemented and what other factors might explain outcomes. Quarterly reports and case studies provided textual accounts of what was happening. The purpose of much of the process evaluation in this study was to ensure that the protocol was implemented according to the protocol. However, the same methods could be used in any program which needs to monitor the implementation process.

Implications

One of the difficulties that arises with this type of evaluation is that it is easy to collect large amounts of data which are then very difficult to interpret. Evaluation questions should be developed carefully in the early stages of the process and care taken to ensure that staff needed to do this type of evaluation are available.

The skills for formative evaluation are often not available. However, these strategies may often be the most appropriate level for evaluation, particularly as resources previously tested are adapted for special populations.

Evaluation question 3.

Are the activities occurring according to the plan? Are they appropriate people participating at desirable rates?
(Observational studies, Monitoring, Process Evaluation)

Flora et al, 1988

A collaborative paper written by representatives of the three CVD risk reduction studies describes an approach to monitoring program activities⁽⁸⁾. The COMMIT process evaluation described above is also an excellent example of what is possible to set up when resources for this are available⁽⁶⁾.

Implication

These systems are labour intensive requiring careful gathering of data, data entry and someone who can analyze the data and produce reports which are useful to program planners. This type of monitoring can also act as a pre-post measure of activity at the community level.

Evaluation question 4.

Did those who participated in the intervention change their knowledge, attitudes, behaviours, policies, environments?

(Descriptive, Observational studies which may follow those who participate or may do Cross-sectional surveys before and after intervention).

Many studies⁽⁹⁻¹⁵⁾ assess whether there were changes in knowledge, attitudes or behaviour in those who participated in a program. These are useful since this level of evaluation is often the only one available given lack of resources for comparison or control groups.

Perez-Stable, 1993, Marin, 1994)

The Perez-Stable study^(11,16) tested the impact of a smoking cessation intervention developed for Spanish-speaking Hispanics. The 19 month intervention was directed toward 7667 people in the San Francisco area and was conducted almost exclusively in Spanish.

There were 4 cross-sectional telephone surveys of adults (18-65): 2 before intervention (1986, 1987) & 2 after (1988, 1989). The sample was drawn through random digit dialing from census tracts and the refusal rate was 6% or less. The outcome measures included recognizing programme name, possessing guide, hearing/seeing media messages, knowledge of a raffle and smoking cessation or reduction. Despite samples of almost 2,000 individuals each year, only about 80 people in each survey had quit smoking in the 12 months prior to the survey, and only about 400 additional persons were current smokers. When quitters must be further divided by the amount of exposure to the program, the result is little power to detect an effect of exposure.⁽¹⁶⁾

The major limitation of this type of study is the lack of comparison group. In this instance this weakness was emphasized by the introduction of new state tax which confounded the impact of the intervention. These researchers attempted to deal with this by looking at the reach of the program and relating this to smoking behaviour.

Implications

Even when a study goes to extreme lengths to obtain a representative sample of those exposed to a program, there is no way to control for the impact of external factors. However, much important information is gained by a study of this sort about a special population and how the people respond to a particular intervention. It is important that the evaluation questions reflect these objectives and are not focused only on changes in tobacco consumption.

LANDO 1990

There have been several studies of community contests reported by (Lando)⁽¹³⁾. These usually have no comparison groups but they do track the same group of smokers over time.

In Lando's study, telephone follow-up interviews are the primary measurement tool. In this study, the researchers compare local and statewide responses.

Implication

Contests have wide appeal for increasing awareness and reaching hard to reach smokers. Because a prize is involved, they often have an opportunity to validate self-reported smoking status through bio chemical validation. However, this is only with winners. Results cannot be generalized beyond the setting in which the assessment occurs. However, with this type of intervention, there have been many evaluations in many different settings which assists programmers in deciding whether this approach will be useful in their particular setting.

KVIZ 1992, 1994

This evaluation^(17,18) of a quit smoking cessation program tracked those who participated but also monitored non-participants. They accomplished this by randomly allocating registrants to two panels. One panel was interviewed before and after program, the other was interviewed after only and non-participants identified in calls were followed up 3 - 4 months later.

The 1994 study⁽¹⁸⁾ tests an innovative approach which includes a TV segment, print material and a "buddy system". Through interviews, the investigators examine determinants of success and usefulness of various components. They find that those who use all the elements, do better which is what is almost always found in studies of this nature since more highly motivated smokers tend to take advantage of more resources. However, they recommend that the "buddy system" appears to be an important addition to community-based interventions.

Implication

Studies of this nature focus on the determinants of program success not on impact of the program. The emphasis on non-participants assists program planners to adjust their marketing approach to attract and sustain the interest of this group. Measures of association with selected program elements demonstrate what is working for those who participate. In this type of study, it is not possible to generalize beyond the boundaries of the setting and the population.

Evaluation question 5.

What was the impact on smoking cessation within the intended audience and can the changes be attributed to the intervention?

(Quasi-experimental studies with comparison group or community; Randomized allocation to groups within a community including a control group; Randomization of communities.)

There are a number of designs employed to answer this research question. The easiest to conduct is a cohort study which assesses people prior to an intervention and then follows them after the intervention. The addition of a comparison group enhances the confidence that one can have in the interpretation of outcomes. Randomization to intervention and control groups provides the strongest design.

The large CVD risk reduction demonstration projects and the COMMIT trial provide the best examples of efforts to answer this question. We provide examples of this work here but refer the reader to the extensive literature available on these projects.

Fortman et al, 1993;

Fortmann et al⁽¹⁹⁾ describe the smoking cessation element within the context of the Stanford Heart Disease Program which was a comprehensive intervention on CVD risk. The cohort was identified in both the intervention and comparison communities, early in the program and then assessed at 2 year intervals. Those assessed were exposed to a variety of messages and programs in the community but did not participate in a standardized program.

Almost half of baseline respondents did not continue in the cohort and two thirds of these represented emigration not refusal. Those lost to follow-up are believed to more likely be smokers, therefore setting up an important bias. However, the existence of a comparison group with a similar bias assists in interpretation.

This paper examines the interpretation of cohort and cross-sectional data and therefore is useful to those making a decision regarding which design to use.

The COMMIT^(20,21) papers also discuss these methodological issues which are too complex for this document.

The COMMIT Research Group, 1995

The COMMIT trial randomly allocated communities within pairs to intervention and comparison conditions thus controlling for some selection bias⁽⁴⁾. For a description of the statistical implications of this process, see Gail et al⁽²⁰⁾. The intervention had a statistically significant impact on light to moderate smokers but there was no significant difference between intervention and comparison community cessation rates in heavy smokers.

Implications

There are many methodological issues which plague community-based studies which attempt to establish comparison communities. The most basic one is determining comparability i.e. on which variables should one match communities to establish their comparability? All of the CVD risk trials and the COMMIT trial found that the secular trends and measurement effects, brought about such important changes in the comparison communities that the differences between communities becomes too small to detect a statistical difference.

However, when we consider the difficulty of stopping smoking and the time it

appears to take for individuals to make the change, we might conclude that even small differences are important.

The so-called third generation community-based projects such as Shea, described above and ASSIST are putting evaluation resources into much more focused strategies which take approaches tested in these large scale trials and adjust them for special populations. We are in the early stages of developing the technology for these types of evaluations.

McAlister, 1992

This study⁽²³⁾ is a quasi-experimental design conducted in a poor population at the Texas-Mexican border. Four groups were followed for 5 years. The interventions built on the experience within the Stanford CVD Program in which mass media, intensive contact and volunteer network were examined. There was a sophisticated sampling procedure employed as well as lengthy interviews. Conclusions have important content for programmers. However, methodological problems such as loss to follow-up, small sample size etc., make results inconclusive.

COCKBURN 1990

In this Australian Quit campaign⁽²²⁾, the researchers investigated different distribution methods through clinical settings. The end-point was not impact on smokers. Different settings were randomly allocated to receive one of three approaches for distribution of minimal-intervention program kit:

1. educational facilitator with clinical background, set appointments, follow-up visit 6 wks later, (n=86); cost \$AU 142 or 24 times mail approach
2. specifically trained courier delivered to receptionist with explanation, follow-up call to group 6 wks later (N=93); cost \$14 or twice mail approach.
3. via mail with personally addressed letter, with 6 wk follow-up letter (N=93); cost \$6

In this study, assessment of the delivery method occurred by visiting distribution locations and assessing reach. Two follow-up phone calls & reminder letter led to overall return rate of 79%.

Implications

This type of study does not look at the ultimate impact on smokers but stops at an intermediate point. This is very worthwhile since breakdowns in the delivery chain of interventions has large impacts on the intended end users. However, it does not answer the questions about the delivery channel's effectiveness in reaching smokers. One might ask, why did they not ask smokers whether this channel was useful. In a study of this nature, the recruitment of patients into the study is very difficult to accomplish because of the support needed in the clinical offices. In addition, the recruitment itself becomes an intervention.

3.1.4 Conclusions

This chapter has identified a series of evaluation methodologies utilized in the community setting. This analysis has not been comprehensive but has focussed on a cross section of evaluation methodological approaches and issues. A complete analysis of all community based Tobacco Control Interventions would exceed the scope of this contract and may

very well require a separate study unto itself.

Any analysis of community based interventions must remain cognizant of the scope, factors and unique characteristics inherent in a specific community. Our analysis has identified the strengths and weaknesses involved with each identified methodology. In spite of these limitations, the following general conclusions can be made:

The community-based comprehensive programs exemplified by the CVD risk reduction projects and the COMMIT trial, provide a firm foundation for future evaluation of community level initiatives. They have shown us the level of intensity and length of intervention needed to produce changes in risk factors and reduction in morbidity and mortality. From these projects, we have learned about the costs of different kinds of evaluation. We have also learned that it is possible to create a standardized protocol which can be accepted and implemented in different communities at levels which make comparisons possible.

It is clear that evaluations which contribute to our understanding of the process of successful community-based programming are needed as essential next steps. New methods or new combinations of old techniques are needed and the expertise for this may not be readily available. Health Canada is well situated to bring such individuals from different disciplines together in order to build this area of technical assistance throughout Canada.

We need to address the fact that many community-based studies have found that the secular trends and measurement impacts have produced changes in comparison communities that lead to insignificant differences between intervention and comparison communities. This means that evaluations which compare communities need to be selected with care and justified accordingly.

3.1.5 References - Additional Information

1. Jackson, C., Altman, D., Howard-Pitney, B., Farquhar, J., Evaluating Community-Level Health Promotion and Disease Prevention Intervention, Marc T. Braverman (ed.) Evaluating Health Promotion Programs, New Directions for Program Evaluation, no.43 San Francisco : Jossey-Bass, Fall 1989: 19-32
2. Mittelmark, M., Hunt, M., Heath, G. Schmid, T., Realistic Outcomes: Lessons from Community-Based Research and Demonstration Programs for the Prevention of Cardiovascular Diseases, Journal of Public Health Policy, Winter 1993: 437-462
3. Shea S, Basch CE, Lantigua MD, Wechsler H. The Washington Heights-Inwood Healthy Heart Program: A third generation community-based cardiovascular disease prevention program in a disadvantaged urban setting. Preventive Medicine 1992; 21, 203-217.
4. Lichtenstein E, Wallack L, Pechacek T. Introduction to the Community Intervention Trial for Smoking Cessation (COMMIT). Int Q Community Health Educ 1990-91;11:173-185.
5. Mattson M, Cummings K, Lynn W, Giffen C, Corle D, Pechacek T. Evaluation plan for the Community Intervention Trial for Smoking Cessation (COMMIT). Int Q Community Health Educ 1990-91;11:271-290.
6. Corbett K, Thompson B, White N, Taylor M. Process evaluation in the Community Intervention Trial for Smoking Cessation (COMMIT). Int Q Community Health Educ 1990-91;11:291-309.
7. COMMIT Research Group, Community Intervention Trial for Smoking cessation(COMMIT): Cohort results from a four-year community intervention AJPH 1995 Feb.
8. Flora, J., Lefebvre, C., Murray, D., Stone, E., Assaf, A., Mittelmark, M., Finnegan, J., A Community Education Monitoring System: Methods from the Stanford Five-City Project, The Minnesota Heart Health Program, and the Pawtucket Heart Health Program Paper; ?? date: 1-34
9. Clarke V, Hill D, Murphy M, Borland R. Factors affecting the efficacy of a community-based quit smoking program. Health Educ Res 1993;8:537-546.
10. Harvey H, Tuffin A, Tuffin K. An evaluation of a community based antismoking campaign. NZ Med J 1990;103:243-245.

11. Pérez-Stable E, Marín BVO, Marín G. A comprehensive smoking cessation program for the San Francisco Bay Area Latino community: Programa Latino Para Dejar de Fumar. *Am J Health Promotion* 1993;7:430-441.
12. Pierce JP, Macskill P, Hill D. Long-term effectiveness of mass media led antismoking campaigns in Australia. *Am J Public Health* 1990;80:565-569.
13. Lando H, Loken B, Howard-Pitney B, Pechacek, T., Community Impact of a Localized Smoking Cessation Contest, Public Health Briefs. *Am J Public Health* 1990;80:601-605.
14. Cummings KM, Sciandra R, Markello S. Impact of a newspaper mediated quit smoking program. *Am J Public Health* 1987;77:1452-1453.
15. Wheeler RJ. Effects of a community-wide smoking cessation program. *Soc Sci Med* 1988;27:1387-1392.
16. Marin BVO, Marin G, Pérez-Stable EJ, Hauck WW. Effects of a community intervention to change smoking behavior among Hispanics. *Am J Prev Med* 1994;10:340-347.
17. Kviz FJ, Crittenden KS, Warnecke RB. Factors associated with nonparticipation among registrants for a self-help, community-based smoking cessation intervention. *Addictive Behav* 1992;17:533-542.
18. Kviz FJ, Crittenden KS, Madura KJ, Warnecke RB. Use and effectiveness of buddy support in a self-help smoking cessation program. *Am J Health Promotion* 1994;8:191-201.
19. Fortmann S, Barr Taylor C, Flora J, Jatulis D. Changes in adult cigarette smoking prevalence after 5 Years of community health education: The Stanford Five-City Project. *Am J Epidemiol* 1993;137:82-94.
20. Gail MH, Byar DP, Fehacek TF, Corle DK. Aspects of statistical design for the Community Intervention Trial for Smoking Cessation (COMMIT). *Controlled Clin Trials* 1992;13:6-17.
21. COMMIT Research Group, Community Intervention Trial for Smoking cessation(COMMIT):Changes in Adult Cigarette smoking prevalence. *AJPH* 1995, February.

22. Cockburn J, Ruth D, Sigagy C, Dobbin M, Reid Y, Scollo M, Naccarella L. Marketing a smoking cessation program to general practitioners for use in clinical practice. Victorian Smoking and Health Program, Quit Evaluation Studies, 1990-1991, No.6, Chapter 9.

23. McAlister AL, Ramirez AG, Amezcua C, Pulley LV, Stern MP, Mercado S. Smoking cessation in Texas-Mexico border communities: a quasi-experimental panel study. *Am J Health Promotion* 1992;6:274-279.

Bibliography

Articles Reviewed For This Chapter

Anderson Johnson C, Pentz MA, Weber MD, Dwyer JH, Baer N, MacKinnon DP, Hansen WB, Flay BR. Relative effectiveness of comprehensive community programming for drug abuse prevention with high-risk and low-risk adolescents. *J Consult Clin Psychol* 1990;58:447-456.

Assaf, A., Banspach, S., Lasaster, T., McKinlay, S., Carleton, R., The Pawtucket Heart Health Program: II. Evaluation Strategies, *Rhode Island Medical Journal*, December 1987: 541-546

Biglan A, Metzler C, Ary D. Increasing the prevalence of successful children: the case for community intervention research. Oregon Research Institute, Paper. ?? date

Bloch M, McLellan D. Women, girls and tobacco. In: Tobacco use: an American crisis. Final report of the conference, January 9-12, 1993, Washington, DC:27-31.

Blum A. The targeting of minority groups by the tobacco Industry. In: Jones LA, ed. *Minorities and Cancer*. New York: Springer-Verlag, 1989:153-162.

Cockburn J, Reid Y. A strategy for evaluating the QUITZ pack. Victorian Smoking and Health Program, Quit Evaluation Studies, 1990-1991, No.6, Chapter 8.

Cohen, R., Stunkard, A., Felix, M., Measuring Community Change in Disease Prevention and Health Promotion, *Preventive Medicine*, 1986; 15: 411-421

Council on Scientific Affairs, American Medical Association. Environmental tobacco smoke: health effects and prevention policies. *Arch Fam Med* 1994;3:865-871.

- Darity WA, Cernada GB, Chen TTL, Pastides H, Stanek EG, Tuthill RW, Winder A. *Cancer prevention (smoking) in the Black population: a community research/intervention model*. In: Jones LA, ed. *Minorities and cancer*. New York: Springer-Verlag, 1989:121-136.
- Elder JP, Campbell NR, Mielchen SD, Hovell MF, Litrownik AJ. Implementation and evaluation of a community-sponsored smoking cessation contest. *Am J Health Promotion* 1991;5:200-207.
- Finnegan, J., Murray, J., Kurth, C., McCarthy, P., Measuring and Tracking Education Program Implementation: The Minnesota Heart Health Program Experience, *Health Education Quarterly*, Spring 1989;16(1): 77-90
- Freedman L, Green S, Byar D. Assessing the gain in efficiency due to matching in a community intervention study. *Statistics in Medicine*, 1990;9:943-952.
- Freeman H, Delgado JL, Douglas CE. Minority issues. In: *Tobacco use: an American crisis*. Final report of the conference, January 9-12, 1993, Washington, DC:43-47.
- Gunning-Schepers L, Barendregt J, Van der Maas P. Population interventions reassessed. *Lancet* 1989;1(8636):479-481.
- Gyorkos, T., Tannenbaum, T., Abrahamowicz, M., Oxman, A., Scott, E., Millson, M., Rasooly, I., Frank, J., Riben, P., Mathias, R., Best, A. An Approach to the Development of Practice Guidelines for Community Health Interventions, *Canadian Journal of Public Health*, July-August 1994; 85 (Supplement 1): S8-S12
- Jooste P. Community anti-smoking intervention in South Africa (editorial). *S Afr Med J* 1991;79:235-236.
- Keintz M, Rimer B, Fleisher L, Fox L, Engstrom P. Use of multiple data sources in planning a smoking cessation program for a defined population. *Advances in Cancer Control: Cancer Control Research and the Emergence of the Oncology Product Line*, 1988:31-42.
- King AC, Flora JA, Fortmann SP, Taylor CB. Smokers' challenge: immediate and long-term findings of a community smoking cessation contest. *Am J Public Health* 1987;77:1340-1341.
- Kofron P, Råstam L, Pirie P, Holder W, Luepker R. Physician practice for cardiovascular disease risk-factor reduction in six upper midwestern communities. *J Fam Med* 1991;32:49-55.

Lacey I, Tukes M, Manfredi C, Warnecke R. Use of lay health educators for smoking cessation in a hard-to-reach urban community. *J Community Health* 1991;16:269-281.

Lando HA, Hellerstedt WL, Pirie PL, Fruetel J, Huttner P. Results of a long-term community smoking cessation contest. *Am J Health Promotion* 1991;5:420-425.

Lefebvre RC, Cobb GD, Goreczny AJ, Carleton RA. Efficacy of an incentive-based community smoking cessation program. *Addictive Behav* 1990;15:403-411.

McCormick J, Skrabanek P. Coronary heart disease is not preventable by population interventions. *Lancet* 1988;ii:839-841.

McGraw, S., McKinlay, S., McClements, L., Lasater, T., Assaf, A., Carleton, R., Methods in Programs Evaluation, The Process Evaluation System of the Pawtucket Heart Health Program, Evaluation Review, October 1989: 459-483

McManus J, Taylor CB, Patrick C, eds. Community approaches to the prevention and cessation of smoking. Stanford Center for Research in Disease Prevention, Health Promotion Resource Centre, 1988.

Millar W, Naegele B. Time to Quit: Community involvement in Smoking cessation. *Can J Public Health* 1987;78:109-114.

Moskowitz, J. Preliminary Guidelines for Reporting Outcome Evaluation Studies of Health Promotion and Disease Prevention Programs. Marc T. Braverman (ed.) Evaluating Health Promotion Programs, New Directions for Program Evaluation, no.43 San Francisco : Jossey-Bass, Fall 1989: 101-112

Mullen, P., Iverson, D., Qualitative Methods for Evaluative Research in Health Education Programs, *Health Education*, May/June 1982: 11-18

Mullins R, Borland R. Evaluation of the 1990 young women's campaign: results from a telephone survey. Victorian Smoking and Health Program, Quit Evaluation Studies, 1990-1991, No.6, Chapter 3.

Myles DR. Stressful life events and cigarette smoking behavior in an adult population which completed a freedom from smoking program [Dissertation]. Carbondale, Illinois: Southern Illinois University, 1986. 115 p.

Pentz M, Anderson Johnson C, Dwyer J, MacKinnon D, Hansen W, Flay B. A comprehensive community approach to adolescent drug abuse prevention: effects on cardiovascular disease risk behaviours. *Ann Med* 1989;21:219-222.

Pérez-Stable E, Marín BVO, Marín G. Smoking cessation community interventions for Latinos. *Wellness Lecture Series*, October 22, 1991; Paper.

Roberts C, Smith C, Catford J. Quit and Win Wales: an evaluation of the 1990 Pilot Contest. *Tobacco Control* 1993;2:114-119.

Ryan B, Coffin K, Smillie C, Porter K. Smoking cessation in Nova Scotia: results of the Time to Quit Program. *Can J Public Health* 1990;81:166-167.

Smokers' beliefs about smoking cessation: 20 U.S. Communities, 1989. *Morbidity and Mortality Weekly Report* 1990;39:653-656.

Steenkamp HJ, Jooste PL, Jordaan, Swanepoel ASP, Rossouw JE. Changes in smoking during a community-based cardiovascular disease intervention programme. *S Afr Med J* 1991;79:250-253.

Thompson, B., Corbett, K., Bracht, N., Pechacek, T., Lessons Learned from Mobilization of Communities in the Community Intervention Trail for Smoking Cessation (COMMIT) Paper, COMMIT Research Group; ?? date: 1-31.

Understanding and intervening with the total population of smokers. *Tobacco Control* 1993;2:95-96.

Wagner E, Schoenbach V, Orleans C, Grothaus L, Saunders K, Curry S, Pearson D. Participation in a smoking cessation program: a population-based perspective. *Am J Prev Med* 1990;6:258-266.

Warnecke RB, Flay BR, Kviz FJ, Gruder CL, Langenberg P, Crittenden KS, Mermelstein RJ, Aitken M, Wong SC, Cook TD. Characteristics of participants in a televised smoking cessation intervention. *Prev Med* 1991;20:389-403.

3.2 Worksite Based Initiatives For Blue Collar Workers

3.2.1 Introduction

It has been documented that smoking levels are higher, interest in quitting is lower and occupational exposures to environmental pollutants are higher among blue collar workers than among other occupations (Covey and Wynder, 1981; Sorenson and Pechacek, 1986; Fielding 1991). Worksites may offer an ideal context in which to implement comprehensive smoking interventions (Gruman and Lynn, 1993). Not only do these efforts afford protection from environmental tobacco smoke (ETS) to nonsmokers, but, also, assistance can be given to those who want to quit, and the establishment of nonsmoking social norms can be enhanced (Sorenson and Pechacek, 1989; Borland et al, 1990b; Borland et al, 1992; Brigham et al, 1994).

Two basic types of interventions have been reported in the literature: policy changes, either at the individual work site level or at the jurisdictional level, and cessation programs (Orleans and Shipley, 1982; Hallett, 1986; Bibeau et al, 1988; Fielding 1991). Smoking policies include restrictions or bans on smoking at the workplace, refusals to hire smokers, certain incentives for smokers to quit, negative sanctions against workers who do not quit, and the removal of cigarette vending machines from company premises. These policies are enacted for several reasons; to protect non smokers from the health effects of environmental tobacco smoke (Biener et al, 1989; Marcus et al, 1992); to reduce potential fire and other damage from smoking; to increase worker productivity and reduce time lost from work. Such policies, however, may also impact on the smoking behaviour itself (Glasgow 1989; Fielding 1991). Smoking cessation programs in the workplace are specifically designed to help employees who smoke to quit or reduce the amount smoked.

These programs have employed many strategies, such as broad based educational programs, self-help materials, group and individual counseling sessions, support groups, aversive therapy, behavioral contracts and other types of behavioral approaches, incentives and competition, and nicotine replacement therapies (Fielding, 1991).

Few worksite smoking initiatives have been specifically designed for and evaluated in blue collar workers. In some outcome evaluations, however, the effects of worksite smoking interventions were determined separately in blue and white collar workers. In most instances, however, evaluations concerned impacts in workers, as a whole.

In the literature search, the vast majority of evaluation reports were from the United States. Because of the close geographic, economic and social ties, findings from studies this country may be more relevant to Canada than those from studies conducted elsewhere. However, since this evaluation was concerned with methodologies rather than findings per se, studies from several countries, namely, the United States, the United Kingdom and Australia, have been included. Reports from several other countries were identified, namely, Germany (Brenner and Mielch, 1992; Brenner and Fleischle, 1994), South Africa (Metcalf and Yach, 1992), and Sweden (Sloan et al, 1990), but are not specifically discussed. As well, in the literature search we concentrated on identifying quantitative evaluations of exposure and behavioural impacts of worksite smoking interventions. However, formative evaluations employing qualitative methods (e.g., Pucci and Haglund, 1992), process evaluations (e.g., Glasgow et al, 1991), cost-effectiveness evaluations (e.g., Erfurt et al, 1991), case studies (e.g., Bertera, 1990), and multiple attribute evaluations (conjoint analysis) (e.g., Spoth, 1990), add important perspectives.

While most of the studies reviewed concerned the evaluation of workplace-based smoking interventions per se, some interventions were carried out within the context of more comprehensive workplace-based health risk reduction programs, and, thus, the evaluation of the smoking intervention was part of a more comprehensive evaluation. Several such studies are cited, namely, the Healthy Worker Project (Jeffrey et al, 1993), LIVE for LIFE program (Breslow et al, 1990; Jeffrey et al, 1993), and the Working Well Trial (Abrams et al, 1994). These multidisciplinary workplace programs are directed at health in general and often include weight control, diet and exercise, as well as smoking cessation, among the interventions being tested. As well, in some instances, for example, the COMMIT trial (e.g., Glasgow et al, 1992), the workplace intervention occurred within the context of a broader-based community intervention.

Methodological Issues

We wish to draw attention to several critical reviews of worksite smoking evaluations. Hallett (1986) observed that many published studies were not intended as evaluations, and, of those that were, many had serious procedural or methodological flaws. He identified and critically evaluated nine experimental or quasi-experimental studies and 12 non-experimental studies of workplace smoking interventions, mainly cessation programs. Bibeau et al (1988) reviewed 21 published evaluations of workplace smoking cessation programs, of which only one third had an experimental (random assignment to treatment and control groups) or quasi-experimental design (non-random control group). The rest were described as pre-experimental, employing pre- and post-intervention measures, but with no control or comparison group against which to gauge the changes in the treatment group. Matson et al (1993) identified and critically evaluated 30 studies of the impact of incentives and competitions on participation and quit rates in worksite smoking cessation programs. Fifteen did not have comparison groups. Of the 15 which employed a quasi-experimental or experimental design, only 8 had a comparison group which permitted the effects of incentives and competition to be separated from the effects of other interventions, and only one study separated the effects of incentives from competitions. Most of 8 studies measured only short-term impact; 3 reported evaluations 12 months after the intervention. Several other reviews have addressed methodologic issues as they relate specifically to workplace smoking interventions (Biener et al, 1989; Glasgow, 1989; Fielding, 1991). Biener et al (1989) provided examples of steps taken to

address specific issues in their study of a worksite intervention in a hospital setting.

There are clearly some methodological issues that arise specifically in the context of workplace smoking policy and program interventions. First, are the statistical considerations that occur when programs and/or policies are introduced in worksites. Many reports do not address the issue of clustering, which can have considerable impact on results (e.g., Kelder et al, 1993), in spite of the fact that worksites are randomly assigned to receive specific conditions. Related to this is the lack of random assignment of worksites or individual workers in many evaluations. Even when worksites or individuals are randomly assigned to conditions, those included frequently are those agreeing to participate, thus calling into question the external validity of some of the findings.

Another consideration is the variety of outcomes that might to be addressed in any worksite evaluation. The range of potential outcomes is large and, often, only a few are considered. For example, in smokers, behavioral changes can range from modification and/or complete cessation of smoking at work, to changes in smoking during nonwork hours and attempts at cessation. There may be direct effects on the health of both smoking and nonsmoking workers from reduced exposure to ETS and satisfaction with policies, as well as increased productivity and reduced absenteeism (Glasgow, 1989). Social norms and smoking-related attitudes in both smokers and nonsmokers may be changed (Glasgow, 1989). Also important to consider are such mediating factors as self-efficacy, perceived social support, job stress, attitudes toward smoking and stages of change (Sorenson et al, 1986; Brown et al, 1988; Curry et al, 1989; Glasgow, 1989; Green and Johnson, 1990). In addition, the reliability and validity of the outcome measures are frequently overlooked. Low response rates in worksite surveys (Thompson et al, 1991) and high attrition in cohort studies over the follow-up period may introduce bias and reduce statistical power. Assessment of impact in the longer-term are often not undertaken.

Another, often ignored, consideration is the organizational context in which the intervention occurs. Variables such as the size and structure of the worksite, the existence of unions, the source of the policy/program be it employer or employee implemented and designed, and the strategies available for enforcement or implementation are all relevant to the impact of the program (Brown et al, 1988; Sorenson et al, 1991). It has been suggested that different types of programs are differentially effective for management as compared to nonmanagement employees, partly because of differences in the mechanisms of and norms for smoking in the different groups, for example, blue collar and white collar workers (Schilling et al, 1985; Serxner et al, 1991). Hence, input from all groups may be important for some program and/or policy interventions.

3.2.2 General Strategies

Evaluation Methodologies Used to Assess Worksite Smoking Interventions

1. Descriptive Studies

Descriptive studies, in which a control or comparison group in neither specified nor implied, document the occurrence of variables of interest (for example, smoking in the workplace, restrictive policies on smoking in the workplace, smoking cessation programs in the workplace) in terms of amount and distribution by persons, place and time. Thus, they permit trends over time to be evaluated, allow comparisons across subgroups or

subsections of the population to be made, and provide a basis for the planning, provision, and evaluation of health services. While they are not intended to be hypothesis testing studies, they may give rise to hypotheses that can be tested in analytic studies.

2. Prevalence Studies

In prevalence or cross-sectional evaluation studies the association between the intervention of interest (e.g., worksite smoking restrictions) and the impact or outcome of interest (e.g., amount smoked) are assessed at the same point in time. Because "cause" and "effect" are measured simultaneously, it is not possible to establish the temporal sequence necessary to draw causal inferences. For example, in a prevalence study it may be demonstrated that decreased rates of smoking are associated with workplace restrictions. However, it cannot be determined from this design whether workplace restrictions preceded decreased rates of smoking or decreased rates of smoking preceded workplace restrictions. None-the-less, prevalence studies are relatively easy and rapid to accomplish, and by providing evidence of an association between two conditions, justify further exploration of the association by more rigorous methods.

3. Pre- and Post-Intervention Cross-Sectional Studies

Pre- and post-intervention cross-sectional studies permit a more direct evaluation of the causal impact of an intervention, by examining change over time in outcome measures in the population or group experiencing an intervention. To accomplish this, independent observations are made before and after the intervention. The evaluation is strengthened if the pre-post change in the population experiencing the intervention can be compared with the secular change in a comparable control population that did not experience the intervention. Often this is not the case, and general population trends are used for comparison purposes, or no attempt is made at all to estimate secular patterns.

4. Post-Intervention Cross-Sectional Surveys

In many instances evaluation of a workplace smoking intervention has been attempted using only post-intervention cross-sectional data, with or without a control group. In such cases, data on the pre-intervention status of variables of interest may be collected retrospectively from each respondent. Such reports may be subject to recall bias. As well, an attempt may be made to ascertain if and how much of the apparent change is attributed by respondents to the intervention.

5. Prospective (Cohort) Studies

The design offers even stronger evidence concerning the direction of cause and effect than the pre-post cross sectional approach and is greatly strengthened by the addition of a comparable control group not exposed to the intervention, but assessed in the same manner. In this design the same individuals are assessed before and after the intervention. The pre-post individual responses are linked and the changes assessed are aggregates of individually based changes. Selective attrition of subjects between pre- and post-assessment may bias findings and lessen statistical power. Like the pre-post cross-sectional approach, this design is greatly strengthened with regard to cause and effect attributions by the addition of a comparable control group not exposed to the intervention, but assessed in the same manner.

6. Randomized Controlled Trials

By far the strongest methodology for evaluating the impact of an intervention is the experimental study or randomized controlled trial. This methodology may involve either the randomization of whole populations or communities or the randomization of individuals to experimental (intervention) and control situations. Random allocation

permits chance alone to determine the assignment of subjects to intervention and control groups. Compared with other methods of forming intervention and control groups, its main advantages are that it eliminates selection bias on the part of participants and investigators, it tends to create groups that are comparable on factors that may influence outcome, whether known or not, and it gives validity to the statistical treatment of the data.

3.2.3 Studies - Outcome Measures

Descriptive Studies

We cite seven descriptive studies (four with direct reference to blue collar workers) regarding smoking and ETS exposure in the workplace, their determinants, and workplace smoking interventions (policies and programs), illustrating a range of objectives. In a 1988 national household interview survey in the United States (Centers for Disease Control, 1992) descriptive information was obtained from employed persons on the prevalence of workplace smoking and on the degree of discomfort (if any) caused by cigarette smoke in workplaces with minimal restrictions. The results permitted a national estimate of workers exposed to tobacco smoke in such workplaces and permitted discomfort from ETS to be characterized according to personal characteristics such as smoking status, sex, age, race, education, and occupational category. With regard to the latter, nonsmoking blue collar workers were less likely to report discomfort than were nonsmoking white collar workers.

With regard to workplace smoking cessation interventions, Sorensen et al (1986) described the relationship of worksite and occupational norms about smoking to workers' attitudes toward smoking cessation. In this study workers of randomly selected workers from worksites in Minneapolis workers in blue collar and white collar occupations were directly compared. Emmons et al (1994) reported descriptive findings on blue collar workers who volunteered for a self-help physical activity intervention and undertook a baseline assessment of health risks. This descriptive analysis permitted a characterization of the smokers (26% of the total sample) according to other risks factors (only 12% of smokers had smoking as their only risk factor) and readiness to change (both with regard to smoking, itself, and other health risks). These findings highlight important issues for research and for the design of programs. Similarly, Abrams and Biener (1992) surveyed all workers in five New England worksites with regard to smoking status, perceptions of workplace smoking norms, perceptions of management concern about smoking, and, for smokers, smoking patterns, indicators of readiness to quit and motivation to quit. White collar and blue collar workers were compared with regard to overall response rate, and smokers within each group were compared with regard to demographic characteristics, readiness to quit indicators, and motivation to quit. Important differences between the groups were identified with direct implications for program interventions, policies, and evaluation strategies.

In order to better understand worker and local union attitudes toward workplace interventions, Brown et al (1988) surveyed union members in a high-risk industry. Attitudes were related to other information collected such as age, smoking status of the respondent, role in the union, receipt of health and safety programs from the union and from the company, supervisor and union support for complaints, supervisor's encouragement of recommended work practices and safety measures, and management concern for occupational health. The findings suggested that certain workplace factors may influence support for workplace smoking control policies.

With regard to the prevalence and characteristics of worksite smoking control activities, two studies provide complementary perspectives. Glasgow et al (1992), in connection with the COMMIT study, surveyed 793 worksites having at least 50 employees selected from the 11 intervention communities (ten in the United States, one in Canada) concerning the prevalence of smoking policies and smoking cessation programs, overall and according to the size of the workplace. Schauflier (1993) surveyed a random sample of corporations in California with more than five hundred employees in order to document the prevalence of policies integrating smoking control into employee benefits. Data were obtained on the prevalence of workplace smoking policies, payment for cessation programs, health insurance coverage for smoking cessation, and the use of smoking status to risk-rate health insurance, and analyzed by size (number of employees) and type of company (construction, wholesale/retail, manufacturing, etc). With regard to risk rating using smoking status and insurance coverage for smoking cessation, information was obtained on whether or not these intervention had ever been considered at all, and whether or not they had been considered and rejected. The findings have direct research and policy development implications.

Prevalence Studies

We cite examples of several hypothesis testing prevalence studies, the first of which provides data directly relevant to blue collar workers. In South Australia, Wakefield et al (1992) examined the relationship between reductions in workday and leisure day cigarette consumption and different levels of smoking restriction (total ban, partial ban, no ban) using representative population data for both blue collar and white collar workers. Differences between leisure day and working day cigarette consumption were examined within each of three worksite smoking conditions across three occupational status categories (high, medium, low). The analysis controlled for sex and number of cigarettes smoked on leisure days. An economic evaluation of the findings in terms of lost revenue to the tobacco industry was also carried out. Kinne et al (1993) used data from a population-based, random digit dialing telephone survey of employed adults in the State of Washington to examine the relationship of smoking status and amount smoked on workdays and nonworkdays to worksite smoking policies (no policy, restrictions, no smoking). Data were analyzed separately for men and women. For current smokers in worksites with either restrictive or nonsmoking policies, respondents were asked about changes in their own smoking, at work and away from work, as a result of the worksite policy, adding a quasi-temporal component. These authors also examined hiring in worksites with policies in relation to smoking status to determine if there was selection on the basis of smoking status. The limitations inherent in the design and data collected are discussed.

Using data collected by telephone interview as part of the 1990 California Tobacco Survey, Woodruff et al (1993) related the responses of 11,704 employed indoor workers with regard smoking status (regular smoking, occasional smoking), amount smoked, desire to quit, and smoking history to four levels of workplace smoking restrictions, ranging from complete bans to no restrictions. Logistic regression was used to control for demographic variables known to be related to smoking status (age, education, ethnicity and sex). The results of the study were strengthened by the demonstration of a clear dose-response relationships between degree of restrictions and the percentage of regular smokers (but not occasional smokers), the amount smoked by both regular and occasional smokers, and a trend for previous occasional smokers to remain so rather than become regular smokers. In this study an economic evaluation of the findings in terms of lost revenue to the tobacco industry was also carried out. Using data from the same survey,

Borland et al (1992) related ETS exposure of nonsmokers, assessed by reports of someone recently smoking in their work area, to the restrictiveness of workplace policies. Logistic regression was used to control for potential confounding variables. Also using data from the same survey, supplemented by separately collected data on local workplace smoking ordinances in California, Pierce et al (1994) related the existence and extent of ordinances (no ordinance, weak ordinance, strong ordinance) to the exposure of nonsmoking indoor workers to ETS, as indicated by the restrictiveness of policies in the workplace (total ban, work area ban, lesser bans or none). Further, the authors were able to compare the association of ETS exposure with the interventions (ordinances and workplace policies) separately and in combination. The cohesion of the findings from the three separate analyses increases confidence in the findings overall.

Pre- and Post-Intervention Cross-Sectional Studies

Gortmaker et al (1987) surveyed all faculty, students and staff of the Harvard School of Public Health four months before and eight months after the implementation of a policy prohibiting smoking in the workplace with the exception of one lounge in the basement. Anonymous respondents to each survey were compared with regard to demographic characteristics, prevalence of smoking, prevalence of smoking during the workday, amount smoked per day at work and at home, bother by someone else's smoking, bother by effects of smoking on clothes and hair smell, frequency of eye irritation, coughing, and headaches, interruption of work performance, and concern for long-term health effects. As well, attitudes toward a total ban on smoking in work areas were assessed, as were self-reported changes in smoking, and attributions of these changes to the policy. Interpretation of the results, however, was limited by the fact that data were not collected on a comparable control group, and, thus, it was not known how much of the change measured reflected secular changes and how much was attributable to the worksite smoking policy. In this instance, the identification of a comparable control group would have been further complicated by the special nature of the population, a group which might be expected to be more aware of health hazards than others. Quit smoking rates were compared with those found in national survey data, both in general, and for those with at least some college education. Stronger evidence for an effect on smoking was provided by the assessment of the attribution of quitting to the policy.

Hocking et al (1991) also used data from a pre-intervention survey and two post-intervention surveys (at 6 and 18 months after the intervention) to assess the impact of a workplace smoking ban in a large telecommunications organization in Australia with regard to smoking behaviour, acceptability of the intervention, and effects on productivity. In this study, pre-post changes in smoking were evaluated against general population trends.

Mullooly et al (1990) used data from a series of anonymous cross-sectional surveys of the nonphysician employees of a large medical care organization to assess changes in employees' smoking behaviour and attitudes following implementation of smoking bans at eleven worksites in 1985 or 1986. All worksites had had three or more pre-ban surveys over a number of years and one or two post-ban surveys that collected demographic data and information on smoking prevalence, attempts to cut down in the last 12 months, and percentage of respondents reporting smoke in the work environment. For these outcomes, time series analyses were carried out. Logistic regression models controlling for year, age, sex, education, and job class were used to compute expected post-ban rates projected from the secular trends in the pre-ban period, assuming no ban effects. The ban effects were assessed by comparing observed and expected post-ban rates. In addition,

for sites implementing bans in 1986, data from surveys conducted immediately before and after the ban (1987) were compared directly using multiple logistic and linear regression models to control for differences in the demographic and job characteristics of the respondents. In these surveys additional questions had been added about smoking quantity, attitudes and predicted (pre) and actual (post) behaviour. Because concurrent data were not available from non-ban sites that would allow secular trends to be statistically controlled, the authors used a nonequivalent post-ban and post-ban comparison of the 1985 ban sites to provide evidence against substantial secular changes over the 1986-1987 period. The consistency of findings arising from the two analysis strategies was taken into account in assessing the impact of the intervention.

In some studies pre- and post-intervention cross-sectional data on effectiveness have been complemented and strengthened by data collected using other designs. For example, in a study (Waranch et al, 1993) of the impact of a smoking ban on the employee smoking in a large hospital setting, data on smoking prevalence and on amount smoked were collected on respondents to an employee survey 6 months before the intervention and compared with similar, unlinked data collected by resurveying the initial respondents six months after the intervention. Only 64% of the respondents to the pre-intervention survey (the post-intervention survey population), provided usable responses post-intervention. The possibility exists that, because of the decreased social acceptability of smoking in the work environment following implementation of the smoking ban and the extensive education and cessation programming efforts that accompanied it, smokers were less likely to respond in the post-intervention survey than were non-smokers, or, if they did respond, they may have been reported more favorably on changes in their smoking, than, indeed, was the case. Such response biases would invalidly enhance pre-post differences in smoking behaviour. However, in this study, a cohort of smoking employees who enrolled in a multi-component smoking cessation program prior to the ban was assessed one year after the hospital became smoke free with regard to quit rates, and, as well, participation rates in the multicomponent program were determined for several years before the ban, for the year of the ban, and for the year following the ban. These complementary data added credence to the differences reported in survey data.

Post-Intervention Cross-Sectional Surveys

An example of a cross-sectional post-intervention assessment with a control group is that conducted by Stave and Jackson (1991). They studied the effects of a smoke-free policy introduced at the Duke University Medical Center in comparison to the adjacent University Campus which did not adopt the policy. Three months after the prohibition on smoking went into effect, randomly selected groups of 400 employees from each campus were surveyed by telephone. Both groups of respondents were asked about smoking behaviour at the time of the survey and retrospectively, at the time the policy was announced nine months earlier. Quit rates and changes in amount smoked since the time of the policy announcement were compared. These data were augmented by a follow-up survey 6 months later of smokers and recent ex-smokers identified in the cross-sectional survey, enabling an efficient, longer term assessment of the impact of the intervention on smoking rates. Sorensen et al (1991a,b) surveyed a stratified random sample of employees 20 months after the implementation of a non-smoking policy in the New England Telephone Company with regard to smoking status, reported air quality, awareness of the smoking policy, satisfaction with smoking policy and preference for type of policy. Self-reported overall and long-term quit smoking rates following awareness that the company had or would be establishing a non-smoking policy were calculated and overall quit rates were compared with general population quit rates. A key

impact variable was the proportion of quitting attributed to the implementation of the policy. Another key variable was the self-reported assessment, according to smoking status, of changes in air quality in work areas (where smoking was prohibited by the intervention) and non-work areas (where smoking was confined to designated areas). Other examples of post-intervention cross-sectional evaluations include studies by Peterson et al (1988) and Scott and Gerberich (1989). In both instances self-reported data on pre-policy smoking behaviour determined retrospectively were compared with post-policy self-reported data.

In communities, post-intervention cross-sectional assessment has been used to assess compliance with laws on workplaces. For example, Rigotti et al (1994) assessed workplace compliance with a no-smoking ordinance in Cambridge, Massachusetts by means of telephone surveys of two stratified random samples of city businesses, 3 and 24 months after the law took effect. Employees awareness and approval of the law, as well as knowledge of the law's requirements and compliance with them were measured. Correlates of awareness, approval and compliance with the law were assessed using multiple logistics regression analysis. Rigotti et al (1992) used a cross-sectional post-intervention design both to evaluate compliance with a local no-smoking ordinance and to assess the effect of a randomized community trial of an educational intervention intended to improve compliance with the ordinance. In this study self-report data on compliance were supplemented by direct observation in unannounced visits to worksites. Lewit et al (1993) surveyed a random sample of moderately sized and small businesses (less than 50 employees) in New Jersey obtaining data relevant to assessing the impact of a law enacted several years earlier requiring smoking policies for worksites with more than 50 employees. The time relationship of the institution of policies (before the law, at the time of the law, after the law) was determined and the prevalence of reported restrictions in worksites covered by the law was compared to the prevalence in worksites not covered by the law. Paulozzi et al (1992) also used post-intervention surveys of both employees and employers to evaluate compliance with the Vermont workplace smoking law and to assess its impact on the prevalence and amount of smoking at work. They pointed out that reliance only on employers reports may overestimate compliance effects.

Prospective (Cohort Studies)

A number of studies using this approach were found in the literature, of which we provide details on several, including two with data relevant to blue collar workers. Kilburn and Warshaw (1990) studied male blue collar workers whose jobs had exposed them to asbestos. Active and retired workers belonging to certain unions, who were or had been engaged in construction and shipbuilding, completed a questionnaire on smoking and respiratory symptoms and were given a brief physical examination, including chest x-rays and lung function tests. From this survey current smokers were identified for intervention. The intervention was a personalized letter from the physician, which contained the results of testing and pointed out the abnormalities. Each letter emphasized the greatly increased risk of lung cancer for asbestos-exposed workers who smoke. From 6 to 25 months after the intervention a brief letter was sent to each current smoker with short questionnaire asking about recall of the screening and report letter and current smoking. Fifty letters were returned as not reaching the subject and 12 workers responded that they has already quit smoking at the time of screening. Among the remaining 2,627 workers the response rate was just 19%. Therefore, a randomly selected group of non-respondents was contacted by telephone and interviewed using the same questions that were in the mailed questionnaire. The results were analyzed separately for respondents to the mailed questionnaire and to the telephone interview. Because there

was no control group not receiving the intervention, the reported yearly quit rates over 20 years of the ex-smokers studied in the initial survey were used to estimate the natural quitting rates in this population. In addition, of the mail respondents random samples of subjects who reported either quitting or cutting down were selected for interview again about their smoking 12 months after return of the questionnaire, permitting estimates of both recidivism and longer-term quitting.

Powell (1993) established a cohort of smokers who received a self-help smoking cessation tape. The cohort was examined one year later with regard to smoking rates. There was no comparison group of smokers not receiving the tape. Quit rates were calculated separately for those who reported using the tape and for blue and white collar workers.

Olsen et al (1990, 1991) undertook evaluations of a one year smoking cessation incentive program offered in a large company both at the end of the program period and 5 years after the program was launched. Before the program began a cross-sectional study of all employees was carried out in which smokers were identified and described. All smokers were offered the intervention program; 1,113 enrolled and 1,204 declined. Participants and nonparticipants were different in both their demographic characteristics and on smoking characteristics. In this study the short-term evaluation is an example of an uncontrolled prospective (cohort) evaluation and the long-term evaluation illustrates a controlled prospective evaluation. In the first instance, only the program participants were resurveyed to determine smoking status, to assess efforts that had been made to quit, and, using logistic regression, to determine predictors of cessation. In this evaluation the quit rates was compared with the quit rate in the smoker population during the year prior to the program that was estimated from the cross-sectional survey. In the long-term evaluation both groups of smokers, participants and nonparticipants, were resurveyed five years after the program was offered. Loss to follow-up was minimal, probably reflecting the fact that more than three-quarters of both groups remained employed with the company. Smoking cessation rates, validated biochemically, and duration of cessation were compared in the participant and nonparticipant groups. As well, within the participant group, characteristics of quitters were compared with those nonquitters. Other examples of controlled prospective studies of smoking cessation interventions in workplaces include those by Klesges et al (1986), Maheu et al (1989), and Jason et al (1990), who examined the impact of incentives/competitions, as well as that by Shipley et al (1988), who examined the smoking cessation component of the Johnson and Johnson LIVE for LIFE program.

To evaluate the impact of a restricted smoking policy in a worksite, Brigham et al (1994) assessed changes in smoking, using biological (saliva nicotine and cotinine), subjective (withdrawal symptoms, mood disturbance, work attitude and performance), and behavioural (smoking) measures. The latter measures involved subjective (self-reports of amount and pattern) and objective (collection and measurement of butts) assessment of smoking during three separate periods of the day (end of work to bedtime, arising to beginning work, during work), thus permitting examination of the hypothesis that smokers may compensate for decreases in smoking during work. Smoking employees were recruited from the workplace which was introducing a smoking ban, and from other nearby worksites which did not have, and were not planning, restricted smoking policies. Recruits were examined before and after the ban came into effect on several occasions and were paid for their participation. At baseline the two groups of recruits differed on the number of cigarettes smoked per day (less in recruits from the site where the ban was to be introduced). In an uncontrolled prospective study, Borland et al (1990a,b) evaluated

the impact of a smoking ban on smoking among employees in the Australian public service. Two to four weeks before implementation of the ban a cross-sectional survey was carried out. Five to six months after the ban persons from the first survey, who had put their names on the questionnaire and who were still in the public service, were recontacted. Responses of respondents to this survey were linked to responses in the first survey yielding a matched cohort of 2,113 persons. Besides assessing changes in smoking behaviour among the current smokers, attitudes to the ban were explored across the smoking status groups.

Randomized Controlled Trials

The literature contains many evaluation studies of workplace smoking cessation interventions that used this methodology, but few such studies of policy interventions, except in the context of a comprehensive program intervention, such as that described by Sorensen et al (1993). In this study, eight worksites in Minnesota were randomly assigned to an intervention or comparison condition, after a baseline survey of all employees. The 3-month intervention included consultation for employees on the adoption of a nonsmoking policy, training for nonsmokers to provide assistance to smokers attempting to quit, and cessation classes for smokers. To assess the effects of the intervention, smokers were surveyed 1 and 6 months after it was completed. Outcome measures includes quit attempts, cessation rates, coworker support of previous quit attempts, co-worker discouragement of previous attempts, frequency of coworker requests not to smoke, and co-worker nonsmoking. Multiple regression analysis was used to assess the relationship of cessation and quit attempts to the social norms of the workplace adjusting for treatment group, age, sex, and education. The authors emphasized the need for further research aimed at partialing out the effects of policy and environmental intervention from those aimed at individuals.

Some cessation studies have involved the random assignment of individuals in worksites and others, the random assignment of worksites. The studies of Mallot et al (1984) and Sutton and Hallett (1987) are examples of the former. In the first study, a small group of subjects enrolling in a worksite controlled smoking program in the United States were randomly assigned to the basic program or to the program plus co-worker social support, in which subjects were paired with a supportive partner in the worksite with whom he/she could discuss progress on a daily basis. Both groups were assessed six-months later with regard to changes in smoking behaviour. In the second study 3,253 employees of the head office of a London, England retailing company were surveyed to discern interest in a forthcoming "stop smoking program". Three hundred and thirty-four (334) cigarette smokers indicated interest of whom 270 were randomly selected to receive a personal invitation from the chief medical officer in the company to attend the program and the remaining 64 comprised the no-intervention control group. Of those sent an invitation, 172 actually attended the program, which consisted of two brief consultations and the prescription of nicorette gum. Three groups were constituted for the analysis, invitation-intervention, invitation-did not attend, and control group. Sustained one-year abstinence rates were calculated for each group and the differences in smoking rates between the first two groups combined and the control group was evaluated statistically.

A good example of a well-designed and executed study involving the random allocation of worksite is that conducted by Glasgow et al (1993). They evaluated a year-long incentives-based worksite smoking cessation program in 19 moderately sized worksites. All state worksites employing between 100 and 1,000 workers in either Salem or Portland, Oregon were invited to participate; 19 of the 20 possible worksite agreed to

participate and took part in a baseline health habits survey for employees. Worksites were stratified on the basis of size and prevalence of smoking (determined from the baseline survey) and then were randomly allocated to incentive and no incentive conditions. Using the baseline survey the randomization procedure was evaluated and found to be generally successful on equating conditions on worksite characteristics. All identified current smokers in a worksite were considered as subjects, whether or not they participated (N=1,097). This was the cohort followed to evaluate outcomes. An examination of the baseline characteristics of smokers in the incentive and no incentive conditions showed that they differed in level of education and intention to quit in the next six months, and these factors were adjusted for in the outcome analysis. Several analyses were carried out to evaluate the effects of loss to follow-up. The outcomes variables of interest were the 1-year and 2-year self-reported and biochemically validated smoking cessation rates. These outcome were analyzed in two ways, a conservative analysis of covariance using worksite as the unit of analysis, and a logistic regression analysis using individuals as the unit of analysis. Because of reports in the literature that incentive programs were successful in involving less educated, hard to reach, blue collar workers in change attempts, separate follow-up analyses were conducted to evaluate the effects in employees with some college education compared to those with none.

Other well-designed and executed studies involving randomization by worksite are those reported by Gomel et al (1993), Glasgow (1995), and Abrams et al (1994). In these instances the smoking cessation interventions were part of disease-specific prevention programs, in the first two instances, cardiovascular risk reduction programs, and in the third, a cancer prevention and control program. Gomel et al randomly allocated 28 worksites in Australia to one of four intervention conditions (health risk assessment, risk factor education, behavioural counseling, and behavioural counseling plus incentives). Participants were assessed before the intervention and at 3, 6, and 12 months post-intervention with regard to smoking (cessation and continuous cessation to the 6 and 12 month follow-ups), body mass index, percentage of body fat, aerobic capacity, serum cholesterol and blood pressure. Glasgow et al (1995) have reported on the short-term effects of a low-intensity worksite heart disease risk reduction program involving 26 heterogeneous moderately-sized worksites matched in pairs on key organizational characteristics and randomly allocated to early or delayed intervention conditions. The intervention was multicomponent involving motivational/incentive, educational/skills training, policy/environmental and maintenance elements. The Working Well Trial, now underway, is the largest worksite cancer control trial in the United States involving 114 worksites (57 matched-pairs) and about 25,000 workers accessed thorough four study sites (Abrams, 1994). Since the intervention is targeted at the worksite, the unit of randomization, the intervention, and analysis is consistent and at the level of the worksite, unlike most previously reported worksite health promotion studies.

The final example of a controlled trial of smoking cessation involving worksite randomization is the Healthy Worker Project, a worksite intervention for weight control and smoking cessation (Jeffrey et al, 1993a,b, 1994). Thirty-two worksites in Minnesota were randomized to treatment or no treatment for two years. Treatment consisted of health education classes combined with a payroll-based incentive system. Evaluation was based on cohort and cross-sectional surveys. In this study the "unit of analysis problem", where treatment conditions are allocated and interventions are applied to entire worksites yet data for evaluating health-related outcomes are obtained from individual workers, was specifically studied, including a simulated example of the problem in the context of sites selected on the basis of having a high proportion of blue collar workers (Kelder et al, 1993). As well, the effects of both the existence of restrictive smoking policies at

baseline and their introduction during the intervention period were examined (Jeffrey et al, 1994).

3.2.4 Conclusions

More research is needed specifically focussing on this occupational category. In order to develop content for such interventions, the combination of qualitative and quantitative techniques should be employed, using focus groups, individual interviews and then the strongest possible design to assess effectiveness with regard to a wide variety of outcomes. In addition, the structure of the workplace with regards to occupational groupings, unions, existing policies, norms regarding smoking, national or lower order restrictions on smoking, need to be included so that the content of the evaluation can be specified. This level of information is particularly important when worksites are considering the programme/intervention options that may be relevant, applicable and effective in their particular setting.

Our analysis has involved a search for all available literature on blue collar workers. At the moment, it is not possible to make any specific policy/intervention recommendations based on the available evidence because so few findings have focussed on the group of blue collar workers.

3.2.5 References

Introduction

1. Covey LS, Wynder EL. Smoking habits and occupational status. *J Occupational Medicine* 1981; 23:537-542.
2. Sorenson G, Pechacek TF. Occupational and sex differences in smoking and smoking cessation. *J Occupational Medicine* 1986; 28:360-364.
3. Fielding JE. Smoking control at the workplace. *Annual Review Public Health* 1991; 12:209-34.
4. Gruman J, Lynn W. Worksite and community intervention for tobacco control. in: *Nicotine Addiction - Principles and Management*. CT Orleans, J Slade (eds). New York: Oxford, 1993, Chapter 21.
5. Sorensen G, Pechacek TF. Implementing nonsmoking policies in the private sector and assessing their effects. *New York State J Medicine* 1989; 89:11-15.
6. Borland R, Owen N, Hill D, Chapman S. Changes in acceptance of workplace smoking bans following their implementation: A prospective study. *Preventive Medicine* 1990b; 19:314-322.
7. Borland R, Pierce JP, Burns DM, Gilpin E, Johnson M, Bal D. Protection from environmental tobacco smoke in California. The case for a smoke-free workplace. *J American Medical Association* 1992; 268:749-752.
8. Brigham J, Gross J, Stotzer ML, Felch LJ. Effects of a restricted work-site smoking policy on employees who smoke. *American J Public Health* 1994; 84:773-778.
9. Orleans CS, Shipley RH. Worksite smoking cessation initiatives: Review and recommendations. *Addictive Behaviors* 1982; 7:1-16.
10. Hallett R. Smoking intervention in the workplace: review and recommendations. *Preventive Medicine* 1986; 15:213-231.
11. Bibeau DL, Mullen KD, McLeroy KR, Green LW, Foshee V. Evaluations of workplace smoking cessation programs: A critique. *American J Preventive Medicine*. 1988; 4:87-95.
12. Biener L, Abrams DB, Emmons K, Follick MJ. Evaluating worksite smoking policies: Methodologic issues. *New York State J Medicine* 1989; 89:5-10.
13. Marcus BH, Emmons, KM, Abrams DB, Marshall RJ, Kane M, Novotny TE, Etzel RA. Restrictive workplace smoking policies: Impact on nonsmokers' tobacco exposure. *J Public Health Policy* 1992; 13:42-51.
14. Glasgow RE. Assessment of smoking behavior in relation to worksite smoking policies. *New York State J Medicine* 1989; 89:31-34.
15. Brenner H, Mielck A. Smoking prohibition in the workplace and smoking cessation in the Federal Republic of Germany. *Preventive Medicine* 1992; 21:252-261.

16. Brenner H, Fleischle B. Smoking regulations at the workplace and smoking behaviour: A study from Southern Germany. *Preventive Medicine* 1994; 23:230-254.
17. Metcalf CA, Yach D. Smoking policies in the workplace in the western Cape. *South African Medical Journal* 1992; 81:23-26.
18. Sloan RP, Dimberg L, Welkowitz LA, Kristiansen MA. Cessation and relapse in a year-long workplace quit-smoking contest. *Preventive Medicine* 1990; 19:414-423.
19. Pucci L, Haglund BJA. Focus groups: A tool for developing better health education materials and approaches for smoking intervention. *Health Promotion International* 1992; 7:11-15.
20. Glasgow RE, Hollis JF, Pettigrew L, Foster L, Givi MJ, Morrisette G. Implementing a year-long worksite-based incentive program for smoking cessation. *American J Health Promotion* 5:192-199, 1991.
21. Erfurt JC, Foote A, Heirich MA. The cost-effectiveness of work-site wellness programs for hypertension control, weight loss, and smoking cessation. *J Occupational Medicine* 1991; 33:962-970.
22. Bertera RL. Planning and implementing health promotion in the workplace: A case study of the Du Pont Company experience. *Health Education Quarterly* 1990; 17:307-327.
23. Spoth R. Multi-attribute analysis of benefit managers' preferences for smoking cessation programs. *Health Values* 1990; 14:3-15.
24. Jeffrey RW, Forster JL, French SA, Kelder SH, Lando HA, McGovern PG, Jacobs DR, Baxter JE. The Healthy Worker Project: A work-site intervention for weight control and smoking cessation. *American J Public Health* 1993; 83:395-401.
25. Jeffrey RW, Forster JL, Baxter JE, French SA, Kelder SH. An empirical evaluation of the effectiveness of tangible incentives in increasing participation and behavior change in a worksite health promotion program. *American J Health Promotion* 1993; 8:98-100.
26. Breslow L, Fielding J, Herrman AA, Wilbur CS. Worksite health promotion: Its evolution and the Johnson & Johnson experience. *Preventive Medicine* 1990; 19:13-21.
27. Abrams DB, Boutwell WB, Grizzle J, Heimendinger J, Sorensen G, Varnes J. Cancer control at the workplace: The Working Well Trial. *Preventive Medicine* 1994; 23:15-27.
28. Glasgow RE, Sorensen G, Corbett K. Worksite smoking control activities: Prevalence and related worksite characteristics from the COMMIT study, 1990. *Preventive Medicine* 1992; 21:688-700.

Methodologic Issues

29. Hallett R. Smoking intervention in the workplace: review and recommendations. *Preventive Medicine* 1986; 15:213-231.
30. Bibeau DL, Mullen KD, McLeroy KR, Green LW, Foshee V. Evaluations of

workplace smoking cessation programs: A critique. *American J Preventive Medicine*. 1988; 4:87-95.

31. Matson DM, Lee JW, Hopp JW. The impact of incentives and competitions on participation and quit rates in worksite smoking cessation programs. *American J Health Promotion* 1993; 7:270-295.

32. Biener L, Abrams DB, Emmons K, Follick MJ. Evaluating worksite smoking policies: Methodologic issues. *New York State J Medicine* 1989; 89:5-10.

33. Glasgow RE. Assessment of smoking behavior in relation to worksite smoking policies. *New York State J Medicine* 1989; 89:31-34.

34. Fielding JE. Smoking control at the workplace. *Annual Review Public Health* 1991; 12:209-34.

35. Kelder SH, Jacobs DR, Jeffrey RW, McGovern PG, Forster JL. The worksite component of variance: design effects and the Healthy Worker Project. *Health Education Research* 1993; 8:555-566.

36. Sorenson G, Pechacek T, Pallonen U. Occupational and worksite norms and attitudes about smoking cessation. *American J Public Health* 1986; 76:544-549.
37. Brown ER, McCarthy WJ, Marcus A, Baker D, Froines JR, Dellenbaugh C, McQuiston T. Workplace smoking policies: Attitudes of union members in a high-risk industry. *J Occupational Medicine* 1988; 30:312-320.
38. Curry S, Thompson B, Sexton M, Omenn GS. Psychological predictors of outcome in a worksite smoking cessation program. *American J Preventive Medicine* 1989; 5:2-7.
39. Green KL, Johnson JV. The effects of psychological work organization on patterns of cigarette smoking among male chemical plant employees. *American J Public Health* 1990; 80:1368-1371.
40. Thompson B, Bowen DJ, Croyle RT, Hopp HP, Fries E. Maximizing worksite survey response rates through community organization strategies and multiple contacts. *American J Health Promotion* 1991; 6: 130-136.
41. Sorenson G, Rosen A, Pinney J, Rudolph J, Doyle N. Work-site smoking policies in small businesses. *J Occupational Medicine* 1991; 33:980-984.
42. Schilling RF, Gilchrist LD, Schinke SP. Smoking in the workplace: Review of critical issues. *Public Health Reports* 1985; 100:473-477.
43. Serxner S, Catalano R, Dooley D, Mishra S. Tobacco use: selection, stress, or culture? *J Occupational Medicine* 1991; 33:1035-1039.

Descriptive Studies

44. Centers for Disease Control. Discomfort from environmental tobacco smoke among employees at worksites with minimal smoking restrictions - United States, 1988. *Mortality and Morbidity Weekly Report*. 1992; 41:351-354.
45. Sorensen G, Perhacek T, Pallonen U. Occupational and worksite norms and attitudes about smoking cessation. *American J Public Health* 1986; 76: 544-549.
46. Emmons KM, Marcus BH, Linnan L, Rossi JS, Abrams DB. Mechanisms in multiple risk factor interventions: Smoking, physical activity, and dietary fat intake among manufacturing workers. *Preventive Medicine* 1994; 23:481-489.
47. Abrams DB, Beiner L. Motivational characteristics of smokers at the workplace: A public health challenge. *Preventive Medicine* 1992; 21: 679-687.
48. Brown ER, McCarthy WJ, Marcus A, Baker D, Froines JR, Dellenbaugh C, McQuiston T. Workplace smoking policies: Attitudes of union members in a high-risk industry. *J Occupational Medicine* 1988; 30:312-320.
49. Glasgow RE, Sorensen G, Corbett K. Worksite smoking control activities: Prevalence and related worksite characteristics from the COMMIT study, 1990. *Preventive Medicine* 1992; 21:688-700.
50. Schauffler HH. Integrating smoking control policies into employee benefits: A

survey of large California corporations. *American J of Public Health* 1993; 83:1226-1230.

Prevalence Studies

51. Kinne S, Kristal AR, White E, Hunt J. Work-site smoking policies: Their population impact in Washington State. *American J Public Health* 1993; 83:1031-1033.

52. Wakefield MA, Wilson D, Owen N, Esterman A, Roberts L. Workplace smoking restrictions, occupational status, and reduced cigarette consumption. *J Occupational Medicine* 1992; 34:693-697.

53. Woodruff TJ, Rosbrook B, Pierce J, Glantz SA. Lower levels of cigarette consumption found in smoke-free workplaces in California. *Archives Internal Medicine* 1993; 153:1485-1493.

54. Borland R, Pierce JP, Burns DM, Gilpin E, Johnson M, Bal D. Protection from environmental tobacco smoke in California. The case for a smoke-free workplace. *J American Medical Association* 1992; 268:749-752.

55. Pierce JP, Shanks TG, Pertschuk M, Gilpin E, Shopland D, Johnson M, Bal D. Do smoking ordinances protect non-smokers from environmental tobacco smoke at work? *Tobacco Control* 1994; 3:15-20.

Pre- and Post-Intervention Cross-Sectional Studies

56. Gortmaker SL, Cleary PD, Neville DB, Sobol AM. Evaluation of a smoking prohibition policy in a school of public health. *Smoking Behaviour and Policy Discussion Paper Series*. Institute for the Study of Smoking Behavior and Policy, John F. Kennedy School of Government, Harvard University, December 1987.

57. Hocking B, Borland R, Owen N, Kemp G. A total ban on workplace smoking is acceptable and effective. *J Occupational Medicine* 1991; 33:163-167.

58. Mullooly JP, Schuman KL, Stevens VJ, Glasgow RE, Vogt TM. Smoking behavior and attitudes of employees of a large HMO before and after a work site ban on cigarette smoking. *Public Health Reports* 1990; 105:623-628.

59. Waranch HR, Wohlgemuth WK, Hantula DA, Gorayeb R, Stillman FA. The effects of a hospital smoking ban on employee smoking behaviour and participation in different types of smoking cessation programmes. *Tobacco Control* 1993; 2:120-126.

Post-Intervention Cross-Sectional Studies

60. Stave GM, Jackson GW. Effect of a total work-site smoking ban on employee smoking and attitudes. *J Occupational Medicine* 1991; 33:884-890.

61. Sorenson G, Rigotti N, Rosen A, Pinney J, Prible R. Effects of a worksite nonsmoking policy: Evidence for increased cessation. *American of Public Health* 1991a; 81:202-204.

62. Sorenson G, Rigotti N, Rosen A, Pinney J, Prible R. Employee knowledge and

attitudes about a work-site nonsmoking policy. Rationale for further smoking restrictions. *J Occupational Medicine* 1991b; 33:1125-1130.

63. Peterson L, Helgeson SD, Gibbons CM, Calhoun CR, Ciacco KH, Pitchford KC. Employee smoking behavior changes and attitudes following a restrictive policy on worksite smoking in a large company. *Public Health Reports* 1988; 103:115-119.

64. Scott CJE, Gerberich SG. Analysis of a smoking policy in the workplace. *American Association of Occupational Health Nurses J* 1989; 37:265-273.

65. Rigotti NA, Stoto M, Schelling TC. Do businesses comply with a no-smoking law? Assessing the self-enforcement approach. *Preventive Medicine* 1994; 23:223-229.

66. Rigotti NA, Bourne D, Rosen A, Locke JA, Schelling TC. Workplace compliance with a no-smoking law: A randomized community intervention trial. *American J of Public Health* 1992; 82:229-235.

67. Lewit EM, Botsko M, Shapiro S. Workplace smoking policies in New Jersey businesses. *American of Public Health* 1993; 83:254-256.

68. Paulozzi LJ, Sprengler RF, Gower MA. An evaluation of the Vermont worksite smoking law. *Public Health Reports* 1992; 107:724-726.

Prospective (Cohort) Studies

69. Kilburn KH, Warshaw RH. Effects of individually motivating smoking cessation in male blue collar workers. *American J Public Health* 1990; 80:1334-1337.

70. Powell DR. A guided self-help smoking cessation intervention with white-collar and blue-collar employees. *American J Health Promotion* 1993; 7:325-326.

71. Olsen GW, Shellenberger RJ, Lacy SE, Fishbeck WA, Bond GG. A smoking cessation incentive program for chemical employees: Design and evaluation. *American J Preventive Medicine* 1990; 6:200-207.

72. Olsen GW, Lacy SE, Sprafka JM, Arceneaux TG, Potts TA, Kravat BA, Gondek MR, Bond GG. A 5-year evaluation of a smoking cessation incentive program for chemical employees. *Preventive Medicine* 1991; 20:774-784.

73. Klesges RC, Vasey MM, Glasgow RE. A worksite smoking modification competition: Potential for public health impact. *American J Public Health* 1986; 76:198-200.

74. Maheu MM, Gervirtz RN, Sallis JF, Schneider NG. Competition/cooperation in worksite smoking cessation using nicotine gum. *Preventive Medicine* 1989; 18:867-876.

75. Jason LA, Jayaraj S, Blitz CC, Michaels MH, Klett LE. Incentives and competition in a worksite smoking cessation intervention. *American J Public Health* 1990; 80:205-206.

76. Shipley RH, Orleans CT, Wilbur CS, Piserchia PV, McFadden DW. Effect of the Johnson and Johnson LIVE for LIFE Program on Employee smoking. *Preventive Medicine* 1988; 17:25-34.

77. Breslow L, Fielding J, Herrman AA, Wilbur CS. Worksite health promotion: Its evolution and the Johnson & Johnson experience. *Preventive Medicine* 1990; 19:13-21.
78. Brigham J, Gross J, Stotzer ML, Felch LJ. Effects of a restricted work-site smoking policy on employees who smoke. *American J Public Health* 1994; 84:773-778.
79. Borland R, Chapman S, Owen N, Hill D. Effects of workplace smoking bans on cigarette consumption. *American J Public Health* 1990a; 80:178-180.

80. Borland R, Owen N, Hill D, Chapman S. Changes in acceptance of workplace smoking bans following their implementation: A prospective study. *Preventive Medicine* 1990b; 19:314-322.

Randomized Controlled Trials

81. Sorensen G, Lando H, Perhacek TF. Promoting smoking cessation at the workplace. Results of a randomized controlled intervention study. *J Occupational Medicine* 1993; 35:121-126.

82. Mallott JM, Glasgow RE, O'Neill HK, Klesges RC. Coworker social support in a worksite smoking control program. *J Applied Behavior Analysis* 1984; 17:485-495.

83. Sutton S, Hallett R. Randomized trial of brief individual treatment for smoking using nicotine chewing gum in a workplace setting. *American J Public Health* 1987; 77:1210-1211.

84. Glasgow RE, Hollis JF, Ary DV, Boles SM. Results of a year-long incentives-based worksite smoking-cessation program. *Addictive Behaviors* 1993; 18:455-464.

85. Gorn M, Oldenburg B, Simpson JM, Owen N. Work-site cardiovascular risk reduction: A randomized trial of health risk assessment, education, counseling, and incentives. *American J Public Health* 1993; 83:1231-1238.

86. Abrams DB, Boutwell WB, Grizzle J, Heimendinger J, Sorensen G, Varnes J. Cancer control at the workplace: The Working Well Trial. *Preventive Medicine* 1994; 23:15-27.

87. Glasgow RE, Terborg JR, Hollis JF, Severson HH, Boles SM. Take Heart: Results from the initial phase of a work-site wellness program. *American J Public Health* 1995; 85:209-216.

88. Jeffrey RW, Forster JL, French SA, Kelder SH, Lando HA, McGovern PG, Jacobs DR, Baxter JE. The Healthy Worker Project: A work-site intervention for weight control and smoking cessation. *American J Public Health* 1993a; 83:395-401.

89. Jeffrey RW, Forster JL, Baxter JE, French SA, Kelder SH. An empirical evaluation of the effectiveness of tangible incentives in increasing participation and behavior change in a worksite health promotion program. *American J Health Promotion* 1993b; 8:98-100.

90. Jeffrey RW, Kelder SH, Forster JL, French SA, Lando HA, Baxter JE. Restrictive smoking policies in the workplace: Effects on smoking prevalence and cigarette consumption. *Preventive Medicine* 1994; 23:78-82.

91. Kelder SH, Jacobs DR, Jeffrey RW, McGovern PG, Forster JL. The worksite component of variance: design effects and the Healthy Worker Project. *Health Education Research* 1993; 8:555-566.

3.3 School Based Smoking Interventions In Countries Other Than Canada

3.3.1 Introduction

The purpose of this section of the report is to provide a review of the evaluation characteristics of school based smoking deterrence/intervention studies and policy based programs located outside of Canada. The literature reviewed is based upon research searches of Smoking and Health, E.R.I.C., and Comprehensive School Health databases, from 1980 to the present. This section will consist of four major areas: First, a brief overview and description of the types of interventions most commonly found in school based smoking programs along with a short description of the theoretical basis for these studies, and a summary of typical outcome measures will be discussed. Second, a short theoretical examination of the four types of validity that must be considered when assessing the merits of a study. Third, an applied summary of the methodological strengths and weaknesses that impact upon the evaluation of these programs. Finally, a more detailed description of several exemplary studies, or study characteristics will be provided.

3.3.2 General Strategies

The types of interventions most commonly found in the literature consisted of two broad areas. Program based interventions that focused on smoking intervention programs, aimed at preventing or delaying smoking initiation, or encouraging cessation in school-aged subjects, formed the bulk of all school based smoking research. Policy based (legislative, or non-legislative) programs, directed at school staff and/or students which mandated specific acceptable or unacceptable smoking related behaviours while in or on school property comprised a much smaller, although significant proportion of the literature.

Contemporary interventions (post 1980's) have largely been based upon social learning theory, and have focused on social influence awareness and refusal skill development ^(1,2). The National Cancer Institute in the United States, in their strategy paper on School Programs To Prevent Smoking: The National Cancer Institute Guide to Strategies that Succeed ⁽³⁾, have cited social influence curriculum as an important component of smoking prevention programs. While the social influences model is the most prevalent in contemporary interventions others do exist. The more traditional information model (health beliefs model) is founded on the belief that providing knowledge regarding the harmful effects of smoking will modify adolescent's attitudes, knowledge and ultimately behaviours towards smoking. The affective model ⁽⁴⁾ assumes smoking behaviour is largely influenced by attitudes, and thus programs are designed to foster a greater sense of self-esteem, and self-image. The Problem Behaviour Theory (Cognitive - Behavioural Model) presumes that adolescents engage in problem behaviours (smoking, drug use) as a means of achieving desirable goals such as peer acceptance, and popularity. The Life Skills Training model, or General/Social Skills based programs ⁽⁵⁾ contain many of the same aspects of the social influence model, yet include some or all of the following additions: Personal skills such as stress management, esteem enhancement, and decision making. Skills to increase self control and self esteem such as self reinforcement, goal setting, and principles of self change. Adaptive coping strategies for relieving anxiety and stress, and general interpersonal skills. The curriculum based on this approach are generally longer than other models, lasting six to twelve sessions.

Most behavioural outcome measures consisted of smoking prevalence or amount of

cigarettes smoked in the previous day, week, or month ⁽⁶⁻⁸⁾. A few policy based studies utilized school personnel estimates of student smoking ⁽⁹⁾. Many studies used biochemical validation of smoking status via expired air carbon monoxide ⁽⁶⁾, or saliva thiocyanate measures ⁽¹⁰⁾. A large number of studies measured knowledge and attitude changes in subjects ^(11,12), and few measured refusal skills employed after program implementation ⁽⁶⁾, and increased resistance to smoke ⁽⁸⁾. Very few studies reported any assessment of program delivery ⁽¹¹⁾.

Validity Issues in Smoking Interventions

Statistical validity concerns itself with any study issue that directly or indirectly inflates error variance:

- 1) large subject heterogeneity within treatment conditions;
- 2) measurement issues, such as imprecise measurement tools, untrained personnel conducting measurements;
- 3) reliability of treatment implementation - accurate dissemination of program curricula or increases probability of making a type I or type II error:
 - 1) Low power due to small sample sizes;
 - 2) Incorrect use of unit of measurement/randomization.

Threats to internal validity are concerned with factors that make the experimental and control groups different, other than the treatment itself:

- 1) Non-randomized groups (more chance treatment and control groups are different on baseline characteristics that may effect outcomes, other than the intervention).
- 2) Selective attrition - if attrition rates are different between groups, it is possible outcomes are due to differences in drop-outs and those that stay in experiment, rather than treatment itself. May affect external validity as well.
- 3) History - outside events affecting the dependent variable (controlled by the inclusion of a control group).
- 4) Maturation Effects - in longitudinal smoking designs subjects are often followed over years; it is possible changes seen in subjects are due to maturational effects rather than the treatment itself, controlled through the use of a control group.
- 5) Compensatory rivalry, demoralization, can occur when several treatments are administered in the same geographical area (i.e., several treatments within the same school).
- 6) Statistical Regression To The Mean - more likely to occur when high or low risk groups are chosen (i.e., social influence models that stratify subjects based on risk).

Construct validity can be defined as the ability to interpret the outcome as being due to the treatment, rather than other variables:

- 1) Inadequate pre-operational definition of construct can lead to differences in treatment implementation. Should have convergence and divergence across measures and manipulations.
- 2) Interaction of treatments, interaction of procedure and treatment. In

- research settings where additional measurement of subjects is occurring (when it will not occur at the same level during the diffusion of the smoking treatment) the additional procedural component of the treatment may be influencing outcomes, rather than just the treatment itself.
- 3) Experimenter expectancies, and hypothesis guessing may be responsible for outcomes in subjects rather than treatments (use of anonymous data collection techniques, consistent treatment implementation minimize these threats).

External validity is threatened when results or outcomes from a study are not generalizable to the intended target group. Threats arise from:

- 1) Temporal effects - effectiveness of a particular treatment is limited to a specific time period.
- 2) Situational effects - outcomes from research are not generalizable to other geographical, cultural, socioeconomic, subject characteristics (age, genders).

After reviewing a number of school based smoking cessation programs, several consistent methodological strengths and weaknesses become identifiable. Our review of the literature indicated the majority of the studies involved pre-intervention, post-intervention between group comparisons, with control groups assessed at similar points in time. Often control groups were made up of classrooms or schools that were currently receiving a standard tobacco education program, to which a new smoking/substance abuse program was compared. Thus small, or statistically insignificant results may not be due to the absence of an impact on behavioural measures, but rather a lack of an improvement over conventional, existing programs.

3.3.3 Studies/Outcome Measures

Methodological Characteristics that Strengthen Studies

Several studies employed biochemical validation of smoking status ^(2,6-8,13,14). Research has demonstrated an increased level of accuracy when respondents know their self report data will be checked (actual validation of saliva thiocyanate, or expired air carbon monoxide levels), or respondents think their saliva or expired air will be checked (bogus pipeline)⁽¹⁵⁾.

Random assignment of treatment and control condition to units (class, school, school board) reduces chances of pre-intervention differences between groups which may confound the interpretation of the outcome measures; many, but not all recent interventions randomized treatments ^(6,7,16-19). In addition, the assessment of the comparability of conditions at baseline, across social, gender, age, grade, and various outcome measures, can demonstrate if randomization has equalized groups, and if not appropriate statistical strategies can be employed. Some current studies compare baseline characteristics ^(6,11).

A select group of studies properly analyzed data based upon the unit of randomization, such as the classroom, or school, rather than at the subject level. When the unit of analysis was the subject appropriate statistical methodology was employed ^(7,8).

Studies where drop-outs are assessed can control for threats to internal validity, and many studies have conducted an analysis of study attrition ^(1,6-8,11,16).

The use of passive consent is common ^(1,2,6-8), which increases study participation, and may prevent the refusal of a high risk subpopulation from participating in the study, as seen when active consent procedures are used.

Follow-ups that last beyond 6 months to 1 year allow evaluation of the program long term outcomes, as it is common for effects to decay with time ^(18,20). In addition the use of booster sessions following the primary interventions seem to increase program effectiveness ^(2,21,22).

Very few programs contained explicit, built in process/implementation evaluation tools to assess the extent of curriculum adherence and similarity across treatment units. Few programs reported training procedures for program deliverers, yet most indicate training was performed at some level ^(6-8,23).

Methodological Weaknesses in Current Studies

Many studies use the student as the unit of analysis, rather than the unit of randomization such as classroom, school, or school board ^(6,13,24). If not controlled for in the analysis, this artificially inflates the study power. In addition many studies have used convenience samples, which may not fully represent the true target population, and thus generalizability is lowered ^(2,13,24).

The statement of clear, objective, quantifiable study objectives and goals allows for clear evaluation procedures, and the inclusion of power (sample size) calculations insuring adequate sample sizes facilitate accurate evaluations (i.e., reasons for program failure - low sample sizes, ineffective or inaccurate diffusion of curriculum). Very few studies report these characteristics, possibly due to space limitations in journals, yet some have ^(1,25).

Studies that have different treatments within the same school risk diffusion of treatment, compensatory equalization, and rivalry threats to internal validity ^(6,13,26). Thus, outcomes may not be solely due to treatments.

Much of the current research provides no clear description of the study population, or target population, making the widespread implementation of the program difficult, and if applied to the incorrect target population, potentially ineffective ^(24,26).

Short follow-up, and small amounts of intervention (i.e., 2 hours of treatment, no booster sessions) make outcome evaluation difficult. This can result in the inability to determine if the curriculum is ineffective, or if the dose of intervention is too low to effect any measurable change ^(13,24,28). In addition when follow-up is short, no determination of the lasting effects of the program can be made, which are particularly important in smoking prevention programs.

A few studies assess the acquisition of knowledge or changes in attitudes regarding smoking, yet fail to measure any behaviour change in smoking itself. The inclusion of measurements in smokeless tobacco use, and other drug use may also be of benefit ⁽²⁴⁾.

Many studies do not incorporate process or outcome evaluation tools and measures, that allow the researchers to determine if and how the program curriculum are affecting subjects which subjects or schools are affected, or what portions of the program are particularly effective or ineffective.

Examination of Exemplary and Novel Interventions

Many of the aforementioned methodological and evaluation based strengths and weaknesses occur in the same smoking intervention programs. Very few studies have been completed to date that offer all around methodological and evaluation based excellence, and exhibit statistically significant outcome measures. Hence, an examination of sections of studies that provide examples of novel or exemplary methodological and evaluation designs will be highlighted below, along with the few all around excellent smoking interventions completed to date.

An examination of Lloyd et al., 1983 ⁽²⁸⁾ provides an superior example of methodological and evaluation based planning of a school based smoking intervention. Sampling 6000 Australian school children from 10 - 12 years of age, Lloyd et al., implemented a social influences based pre - post randomized intervention. Identifying 88 primary schools as their study population, matched on several demographics thought to influence smoking, one of each of the matched pairs was randomly assigned to treatment condition, the other becoming the control group. Prior to any treatment a survey was administered to all schools within both treatments, and later that year the program was begun in treatment schools. The program consisted of nine core educational activities including resisting peer pressure, decision making, and the physiological effects of smoking, that lasted for six weeks, at 1.5 hours per week. An additional three-week revision period of a further 1.5 hours per week. The program was administered by the class teacher, who was trained for one day. The study consisted of eight main groups, a 2x2x2 factorial design consisting of two genders, two grades, and the treatment and control conditions. After one year 99.5% and 95.6% (two grades) provided usable data.

Outcome measures consisted of questionnaires assessing the subject, parent, and teacher attitudes, knowledge, intentions and behaviours in relation to tobacco smoke, alcohol, and drug use. Parent questionnaires sought basic demographic, personal smoking status, and attitudes towards smoking and tobacco advertising. Teacher questionnaires consisted of personal tobacco use, and attitudes towards smoking. Treatment teachers received questioners regarding the use of the educational program and their impressions on its effectiveness. Control school teachers were asked about their use of other smoking prevention materials to establish whether or not the control nature of these schools had been maintained. Survey returns from teachers was 80.5% from treatment schools, and 72% from control schools. Questionnaires were administered under supervision of a research team member.

Although the behavioural results of this study were not statistically significant, the methodological design, and built in evaluation components of this study make it a strong example of a evaluation of a smoking intervention program. Some weaknesses are present, most notably the unit of randomization was the school, yet analysis was conducted upon the individual, and the lack of a biochemical validation of smoking status. However, when reviewing the previous section of this report (Methodological Issues in Evaluation of Interventions) it is clear this study succeeds in meeting many of these criteria.

Murray et al., 1992 ⁽¹⁾, in The Minnesota - Wisconsin Adolescent Tobacco Use Research Project, provide an excellent demonstration of sampling protocol. Consisting of a nested-cross sectional design from 1986 to 1990, 43 to 46 sampling units, comprising approximately 3,600 students, were selected at random in each state. The control group consisted of students that had passed through the seventh and eighth grade before the

legislative initiative to provide state supported tobacco intervention programs in all of Minnesota's school districts. The treatment group consisted of students that passed through the seventh and eighth grade after the legislation was in place. Murray et al., provide a detailed power analysis, and hypothesis which enable clear evaluation of outcome measures. The sampling protocol used the entire ninth grade enrolment, and thus every ninth grade student had the opportunity of being selected. Sampling units were created that contained a set range of students, and where schools had enrolments larger than this range, they were split into two or more units, and where smaller ranges occurred schools were randomly grouped with other small enrolment schools to form a unit. Of the 468 Minnesota public schools 694 sampling units were formed. These units were randomized and approached to be included in the study. By including virtually all state public schools generalizability of findings to non-treatment schools within the state is excellent. Although significant effects due to legislation were not found, this could be due to a high level of smoking interventions both before and after legislation occurred.

Ary et al., 1990⁽⁷⁾ demonstrate superior methodological and evaluation based design in their study *The Efficacy of Social-Influence Prevention Programs Versus "Standard Care": Are New Initiatives Needed?* Study strengths included a dual analysis at the subject level and school level, inclusion of schools in both rural and urban centres, diverse socioeconomic representation to increase generalizability, and biochemical validation. Passive consent procedures were used. The study comprised questionnaires to both students, and teachers which involved closed and open ended questions regarding the extent and nature of tobacco/drug education programs in their schools. In addition to a standard in school program, three brochure-like messages were sent, on separate occasions, to parents of students in the social influences treatments. These messages were designed to compliment sections of the in-school presentation and encouraged parents to discuss their attitudes towards tobacco use with their children. Although the prevalence of smoking was similar between both treatments, a significant decrease in number of cigarettes smoked in treatment schools was achieved. Those children of parents receiving parental messages did not differ significantly from parents not receiving messages.

Several reviewed studies contained both novel and important design or evaluation characteristics that merit attention. Tortu et al., 1989⁽²⁷⁾ describe an intervention that failed to achieve the desired treatment effects. Yet upon closer examination of evaluation data, to clear groups emerged that differed on the amount of program curricula that was properly diffused to the subjects. Re-analysis based upon these groups demonstrated that the degree of program implementation is associated with program outcome.

3.3.4 Conclusions

Several methodological strengths and weaknesses become identifiable. Our review of the literature indicated the majority of the studies involved pre-intervention, post-intervention between group comparisons, with control groups assessed at similar points in time. Often control groups were made up of classrooms or schools that were currently receiving a standard tobacco education program, to which a new smoking/substance abuse program was compared. Thus small, or statistically insignificant results in outcome evaluations may not be due to the absence of an impact on behavioral measures, but rather a lack of an improvement over conventional, existing programs.

Very few programs contained explicit, built in process/implementation evaluation tools to assess the extent of curriculum adherence and similarity across treatment units. Few

programs reported training procedures for program deliverers, yet most indicate training was performed at some level ^(6-8,23).

Many studies do not incorporate process or outcome evaluation tools and measures, that allow the researchers to determine if and how the program curricula are affecting subjects, which subjects or schools are affected or what portions of the program are particularly effective or ineffective.

In the area of school based interventions themselves, Silvestri et al., 1989 ⁽⁵⁾ review the success of many large school based interventions, and outline several key curriculum components and recommendations for future interventions. These are; 7-10 class periods (often more than reviewed interventions), a distribution of time that includes 2/3 of time on social issues, and at least 1/2 of the time on social influences and refusal skills; role play and practice of refusal skills should encompass a large section of the program (total of 45% of time), and should be distributed evenly throughout the curriculum; and traditional education about the health consequences of smoking should take about one lesson. They emphasize the most successful programs contain the following key characteristics a) teaching methods that encourage participation, rather than lecture style classes, b) small group activities, c) the use of same age or slightly older peer assistants and actors, d) classroom media (videotapes and movies), and e) some level of parental involvement such as homework activities.

Glynn⁽³⁾ also discusses essential elements of school-based prevention programs in nine areas: program impact, focus, context and length; ideal age at initiation; need for peer and parental involvement; teacher training; and program implementation. These areas were further refined, and existing curricula and programs assessed against them in a national survey sponsored by Health Canada and the Canadian Cancer Society.⁽³¹⁾ One recommendation from this survey was that standardized ways of collecting data on health-related behaviours from youth be developed to monitor changes and guide intervention strategies.

Based on the above discussion of evaluation methodological issues in school-based smoking programs, several issues of relevance to the evaluation of TDRS activities specific to school-based interventions and school-aged youth emerge. These include:

1. Valid standardized measures of smoking behaviour for school-aged youth need to be developed and assessed. It is important that such measures be able to identify youth at various stages of the smoking onset process. Definitions used for adolescents and adults (eg. daily smoking) are not likely to be useful for the majority of elementary school-aged smokers, making it necessary to develop instruments which may be age specific.
2. Related to the above, standardized, rigorous protocols for gathering data in schools need to be developed and implemented in both the context of survey and evaluation research. Issues such as the collection of biochemical data to promote truthful reporting, allowing for the collection of data from students who are absent from initial testing sessions, use of trained data collectors from outside the school, and, in the case of longitudinal designs, tracking of students who move to minimize the effects of attrition, need to be carefully considered for their effects on reported rates and trends.
3. Also related to the above, there is considerable change in smoking onset by single years of age, and this may well vary by gender in school-aged youth. Consequently, data collection in youth needs to be complete enough so that reliable estimates of smoking

prevalence, appropriately defined for the age of the child, can be obtained and reported by single year of age and gender. Alternatively, key time points in the smoking onset process need to be assessed with samples large enough to make precise statements about appropriate rates. For example, the transition to secondary school is one such point, and estimates for before and after this transition are especially important in evaluating efforts to prevent smoking onset.

4. Careful attention must be paid to unit of randomization, unit of intervention and unit of analysis issues. As described above, evaluation research in schools almost always involves either the (random) assignment of schools or classes to condition, or the delivery of interventions to groups, or both. In either case, it is reasonable to expect, and experience confirms, that children within a school/class/group will behave more similarly than will students in another school/class/group either because the students interact (and they may be encouraged to do so), or because factors at the group level (eg. SES or provider of the intervention) influence smoking rates for all students in the group similarly. Evaluation designs and subsequent analyses must recognize this phenomena to achieve adequate power or avoid declaring differences significant when they are not.

5. Careful attention needs to be given to the collection of process data, implementation data, and data on other explanatory variables. Evaluations can achieve greater power with careful attention to blocking (stratification) on variables related to outcome. Analyses can be more efficient and results more informative by carefully considering subpopulations of youth, schools, providers, etc. where interventions may achieve greater (or lesser) success. Indeed, if it were possible to target interventions more effectively, the overall results of TDRS initiatives could be enhanced.

6. In the design of programs and their evaluation, dissemination issues must be considered. This could be achieved by involving those responsible for the eventual implementation and/or dissemination from the outset. Barriers to the effective implementation of school-based programs have been identified in a survey sponsored by Health Canada and the Canadian Cancer Society. There is need for further research to determine ways to put in place and sustain programs of proven effectiveness.

3.3.5 References - Additional Information

1. Rossi, P.H., Freeman, H.E. (1989). Evaluation: a systematic approach. Sage Publications, Newbury Park.
2. Myers, A. (1992). HLTH 604 - Program Evaluation: course notes. University of Waterloo.
3. Glynn TJ: Essential elements of school-based smoking prevention programs: Research results. *J Sch Health* 1989;59(5):181-188.
4. Murray DM, Pirie P, Luepker RV, Pallonen U: Five- and six-year follow-up results from four seventh-grade smoking prevention strategies. *Journal of Behavioural Medicine* 1989;12:207-218.
5. Glynn TJ: School programs to prevent smoking: The national cancer institute guide to strategies that succeed. *U.S.Department of Health and Human Services* 1990;1-24.
6. Botvin GJ, Botvin EM: Adolescent tobacco, alcohol, and drug abuse: prevention strategies, empirical findings, and assessment issues. *Developmental and Behavioral Pediatrics* 1992;13(4):290-301.
7. Silvestri B, Flay BR: Smoking education: Comparison of practice and state-of-the-art. *Prev Med* 1989;18:257-266.
8. Biglan A, Severson H, Ary D, et al: Do smoking prevention programs really work?: Attrition and the internal and external validity of an evaluation of a refusal skills training program. *Journal of Behavioral Medicine* 1987;10:613-629.
9. Ary DV, Biglan A, Glasgow R, et al: The efficacy of social-influence prevention programs versus "standard care": Are new initiatives needed?. *Journal of Behavioral Medicine* 1990;13(3):281-296.
10. Biglan A, Glasgow R, Ary D, et al: How generalizable are the effects of smoking prevention programs? Refusal skills training and parent messages in a teacher administered program. *Journal of Behavioral Medicine* 1987;10(6):613-628.
11. Charlin VL, Sussman S, Dent CW, et al: Three methods of assessing adolescent school-level experimentation of tobacco products. *Evaluation Review* 1990;14(3):297-307.

12. Evans RI: Development of the social inoculation strategy to deter smoking and related health threatening behaviours in adolescents: review and an update, 1990:(UnPub)
13. Botvin GJ, Baker E, Dusenbury L, Tortu S, Botvin M: Preventing adolescent drug abuse through a multimodal cognitive-behavioural approach: results of a 3-year study. *J of Consult and Clin Psych* 1990;58(4):437-446.
14. Rundall TG, Bruvold WH: A meta-analysis of school-based smoking and alcohol use prevention programs. *Health Education Quarterly* 1988;15:3:317-334.
15. Perry CL, Telch MJ, Killen J, Burke A, Maccoby N: High school smoking prevention: The relative efficacy of varied treatments and instructors. *Adolescence* 1983;18(71):561-566.
16. Pentz MA, Brannon BR, Charlin VL, Barrett EJ, MacKinnon DP, Flay BR: The power of policy: The relationship of smoking policy to adolescent smoking. *AJPH* 1989;79:857-862.
17. Glynn SM, Gruder CL, Jegerski JA: Effects of biochemical validation of self-reported cigarette smoking on treatment success and on misreporting abstinence. *Health Psychology* 1986;5(2):125-136.
18. Ringwalt C, Ennett ST, Holt KD: An outcome evaluation of project DARE (Drug abuse resistance education). *Health Education Research* 1991;6:3:327-337.
19. Chadwick JH: Health behaviour change at the worksite: a problem oriented analysis, in Parkinson RSA (ed): *Managing Health Promotion in the Workplace: Guidelines for Implementation and Evaluation*. California, Mayfield Publishing; 1982:144-161.
20. Elder JP, Wildey M, De Moor C, et al: The long-term prevention of tobacco use among junior high school students: classroom and telephone interventions. *AJPH* 1993;83(9):1239-1244.
21. Botvin GJ, Tortu S, Baker E, Dusenbury L: Preventing adolescent cigarette smoking: Resistance skills training and development of life skills. *Special Services in the Schools* 1990;6(1/2):37-61.
22. Puska P, Vartiainen E, Pallonen U, et al: The North Karelia Youth Project: Evaluation of two years of intervention on health behaviour and cvd risk factors among 13-to 15- year old children. *Prev Med* 1982;11:550-570.

23. Klepp KI, Tell GS, Vellar OD: Ten-year follow-up of the Oslo Youth Study Smoking Prevention Program. *Prev Med* 1993;22:453-462.
24. Vartiainen E, Fallonen U, McAlister AL, Puska P: Eight-year follow-up results of an adolescent smoking prevention program : The North Karelia Youth Project. *AJPH* 1990;80:1:78-79.
25. Perry CL, Murray DM, Griffin G: Evaluating the statewide dissemination of smoking prevention curricula: Factors in teacher compliance. *J Sch Health* 1990;60:10:501-504.
26. Banspach SW, Lefebvre RC, Carleton RA: Increasing awareness of the pressures related to smoking: An analysis of two models of anti-smoking curriculum in the classroom. *Health Education Research* 1989;4:69-78.
27. Puska P, Salonen JT, Nissinen A, et al: Change in risk factors for coronary heart disease during 10 years of a community intervention programme (North Karelia project). *British Medical Journal* 1983;287:1840-1844.
28. Telch MJ, Miller LM, Killen JD, Cooke S, Maccoby N: Social influences approach to smoking prevention: the effects of videotape delivery with and without same-age peer leader participation. *Addictive Behaviors* 1990;15:21-28.
29. Tortu S, Botvin GJ: School-based smoking prevention: the teacher training process. *Prev Med* 1989;18:280-289.
30. Lloyd DM, Alexander HM, Callcott R, Dobson AJ, Hardes GR, O'Connell DL, Leeder SR: Cigarette Smoking and Drug Use in Schoolchildre: III-Evaluation of a Smoking Prevention Education Programme. *Int. J. Epid* 1983;12:51-58.
31. Health Canada (1994). School Smoking Prevention Programs: A National Survey. Minister of Supply and Services Canada, Ottawa, Ontario, Canada. (139 pages)

3.4 Evaluating Media Campaigns

3.4.1 Introduction

The purpose of this section is to provide a brief review of methodologies used to evaluate the effectiveness of anti-smoking mass media campaigns carried out by public authorities in countries other than Canada. The emphasis in this section is upon "mass media campaigns (print and electronic) and their effect upon the smoking-related knowledge attitudes and behaviours of specific groups, especially youth age 10-19 of both sexes." Studies assessing outcomes beyond awareness itself are accorded special attention.

Approach:

The reviews in this section build upon two excellent reviews of the literature published by Flay⁽¹⁾ in 1987 and McKillip⁽²⁾ in 1989. In his 1987 review Flay examined evaluations of more than 40 anti-smoking media campaigns conducted in countries such as Greece, Norway, Britain, Austria, and the United States. The McKillip article focuses upon evaluation methodologies - in particular, difficulties in identifying no-treatment baselines for comparison with media effects in health promotion programming. In particular, given the emphasis of this project upon evaluation methodologies, the McKillip article is given detailed consideration.

The first section of the report will summarize some general methodological issues that tend to be generic to the evaluation of media campaigns. The subsequent sections will review three distinct types of media campaigns/evaluation strategies that are designed to effect very different outcomes.

Generally, the evaluation literature relating to mass media campaigns divides broadly into three streams: 1) mass media campaigns directed towards encouraging smoking *cessation*; 2) campaigns which focus upon smoking *prevention*; and 3) campaigns undertaken as part of a broad, comprehensive *multi-component* (eg, ASSIST) or *multi-issue* (eg, Heart Health/ Healthy Lifestyles) intervention. [Note: A fourth stream of media activity that is newly emergent, particularly in California is "media advocacy". This stream has not been incorporated into the current draft, but could be added at a later stage.] Characteristically, each of these three streams of activity are characterized by very different objectives, methods and approaches, intended outcomes (impacts and effects) and evaluation methodologies/issues.

3.4.2 General Strategies

Selected Research Design Evaluation Issues

Self-selection:

In his 1989 review, McKillip identifies two aspects of mass media that complicate evaluation - self-selection of exposure and availability of media to entire groups: "Since most people have a choice of which media messages they will be exposed to, viewing and processing these messages results from an interaction between the media stimulus and characteristics of the audience members. Simple comparisons between those who saw a message with those who did not can confound differences in exposure to health media with differences in interest and knowledge of a topic, in motivation to change, and in need for information on the topic". For, example, studies that focus upon an actual audience (eg, those who respond to a media-based smoking cessation campaign) are complicated by the fact that those who respond, have a much greater motivation and predisposition to smoking cessation than are those who decline to respond. Similarly, studies that compare segments of the potential audience who are exposed vs. not-exposed to a campaign are complicated by selection artifacts as well. For example, if it is determined that people exposed to anti-smoking messages have more positive knowledge, attitudes and beliefs than are those who were not exposed, this may be attributable to pre-existing differences in their interest in the topic or issue, rather than to the media programming itself. However, a recent study by Flay⁽³⁾ examined the effects of defacto (regular viewing habits) and motivated self-selection (watching due to a desire to quit) on smoking cessation outcomes in a televised self-help cessation program. Using nested covariance structure models for the analysis, Flay concludes that: 1) de facto selection had no influence on exposure to the program; 2) motivated selection had no influence on exposure; 3) the program reduced smoking and 4) motivation to quit smoking had the strongest effect upon attempts to quit.

Availability of media to entire groups:

"Television, radio, newspapers and even billboards and bus posters are available across wide geographic areas. Non-exposed groups must come from some other location than the exposed group. Even when the logistical problems associated with finding and measuring distant populations are surmounted, the multiple pre-existing differences between the exposed and the non-exposed groups obscure the inference that observed post-media differences are due to media programming." A recent evaluation study by Laprelle⁽⁴⁾ clearly illustrates this problem. In a study of the influence of three mass media campaigns upon adolescent smoking in a 10 community field experiment high intercommunity variation precluded the detection of media campaign effects, despite careful design and statistical adjustments for a set of 10 known sociodemographic and personality correlates of smoking. Similarly, "spill-over" effects are difficult to rule out given the wide availability of information, and population mobility. Additionally, it is often difficult to ascertain other sources of information exposure, or of exposure to anti-smoking programming in control sites. Finally, the measurement procedures themselves, or in combination with other media exposures may have an independent influence upon outcomes. This may be a particularly serious problems given media campaigns that use an agenda-setting approach⁽⁵⁾, or in

media tracking projects with significant response burdens⁽⁶⁾.

Alternative Research Designs:

These generic design issues must be considered in the evaluation of the mass media component of the Tobacco Demand Reduction Strategy. McKillip offers examples of two research designs that can overcome these limitations.

Multiple Baselines Against Behaviours:

1. Multiple, independent behaviours are observed for a single subject or unit of observation (eg, audience; target population; community) during the entire study.
2. After relatively stable observed baselines are established, an intervention is introduced abruptly to produce changes on one of the targeted baseline measures (eg, knowledge). If the targeted measure changes, while the other baseline measures remain stable, support is provided for the impact of the intervention.
3. A second intervention is abruptly introduced to produce changes on another baseline measure. If this measure changes, while others remain stable, further support is provided.

Control Construct Designs:

1. Multiple constructs are observed during the entire length of the study, including measures related to the media behaviour (smoking) and measures of similar constructs that could but will not be the focus of media intervention (eg, alcohol consumption; exercise).
2. Measures are taken at regular intervals before, during and after media programming, with a different sample of the study population measured at each observation time.
3. Media programming is introduced abruptly in a way that allows modelling of potential impacts.

The specific form of the Construct x Time of Observation interaction expected for replication of the control construct design is tested in an analysis of variance or similar statistical analysis format.

Other Designs:

Other designs not discussed by McKillip, in particular time series designs, and comparative-effectiveness designs will be discussed and illustrated in subsequent sections.

Selected Measurement Issues:

There are a number of measurement issues that are either specific to the realm of media campaign evaluations, or of special interest/concern because of their relationship to campaign activities and objectives.

Self-report:

Self-reports of tobacco use are of concern to all types of evaluation approaches, and objective validations are universally preferred and recommended. However, the issue of self-report bias can be exacerbated in the area of media campaign evaluation in the following ways:

1. Social marketing campaigns frequently seek to influence the social acceptability/desirability of smoking, and by extension, of self-identification as a smoker. A lower reported rate of smoking may reflect only a change in the social desirability of such a self-identification.
2. Campaigns that seek to inform the public about new regulations (eg, sales to minors) also may influence the accuracy of reporting and self-identification, particularly when the behaviour is not legal.

Aided and Unaided Recall:

Perhaps the most common measure of media outputs/impacts/and even, effects derives from the use of aided or unaided recall of campaign messages. These measures are notorious for their questionable validity. A study by Brown⁽⁷⁾ measured recognition of radio and television messages in 3 mass media campaigns designed to keep adolescents from starting to smoke cigarettes in six treatment and four control sites. A telephone survey of 574 adolescents found high recognition of campaign messages even in the areas where the campaigns had not been broadcast.

Stages of Change:

The vast majority of campaigns, and by extension outcome measures used in media campaign evaluations tend to utilize variations on a smoke/don't smoke dichotomy. Recent work by Prochaska and colleagues suggests that far more complex change models and measurement systems are required⁽⁸⁻¹⁶⁾. Prochaska argues convincingly that modification of addictive behaviours (eg, smoking) or adoption of healthy behaviours (eg, exercise) involves progression through five stages: precontemplation, contemplation, preparation, action and maintenance - and individuals typically recycle through these stages several times before termination of the addiction. Depending upon the stage of change, the motivations and supports required to encourage change (eg, knowledge/attitudes/beliefs), and by extension, the measures of "successful" outcomes will vary substantially. Several examples of the application of this model to smoking behaviours suggest strongly that far more sophisticated measurement approaches can and should be used in the monitoring and tracking of mass media smoking cessation and smoking prevention programs.

3.4.3 Studies/Outcome Measures

Media Campaigns And Smoking Cessation:

The starting point for this description is a review by Flay⁽¹⁾ of 40 evaluated anti-smoking campaigns: 9 of which were information/motivation campaigns; 20 were mass media self-help clinics and 11 were promotions of specific cessation related activities. Flay's observations regarding the evaluation methodologies used in these studies are highly instructive: "If we had been restricted to only those studies that satisfied the conditions of minimal scientific validity, there would have been very little to review. These minimal requirements should include some level of randomization to experimental conditions, pretested panels, and intensive measurement and analysis of process. Clear inferences about program effects cannot be drawn from any one of the studies to be reviewed when considered alone, because of limitations in research design, with most studies consisting of post-test only and/or single group designs. The reviewed studies tell us little about those for who the different mass media approaches are most effective, or the conditions of viewing/participating under which they are most effective. It is probably neither possible nor useful to determine the relative cost-effectiveness of the different ways of using mass media for smoking cessation. The lack of good comparison groups in most studies makes it necessary to provide a method of comparing the results of these studies with some acceptable baselines."

Based upon the notion that "a mass media program with effects no greater than what is occurring in the population is probably not of much value, and a mass media clinic that includes printed material will need to perform better than the printed material alone" Flay develops his "gold standard" for judging campaign effectiveness:

- 1) The natural quitting behaviour of the population of smokers without any particular program;
- 2) The effectiveness of a widely used self-help smoking cessation program for smokers who request it;
- 3) The average effectiveness of face-to-face clinics.

More recently, studies of mass media and smoking cessation have been conducted in a large number of countries, including: Australia⁽¹⁷⁻²¹⁾, Finland^(22,23), Norway⁽²⁴⁾, Switzerland⁽²⁵⁾ and various sites within the United States⁽²⁶⁻³⁰⁾. A number of these projects are of interest from a methodological perspective.

McAlister et.al.⁽²⁶⁾ used a quasi-experimental panel study design to assess the relative effectiveness of two theory based health promotion approaches among residents of U.S. - Mexico border communities. A mass media campaign and a community network were combined to encourage and support smoking cessation. To provide a comparison, a program of intensive contact was also provided for a subset of smokers. This program consisted of individual counselling and referral to other support services requested by the participants. Researchers selected geographic locations matched for size, demographic composition, and geographic proximity and assigned participants into three groups of moderate to heavy smokers: 1) comparison (135 participants) 2) mass media, volunteer work and intensive contact (160 participants) and 3) mass media/ brief contact (124 participants). In 1985 and 1986 baseline data was collected using household interviews of respondents age 16-60. Annual follow-up interviews were conducted over a five year period. Self-reported smoking information was collected from both the Texas and Mexican respondents, and carbon monoxide breath analysis was used to verify self-reported smoking among the Texas participants only. Over the five year period smoking cessation rates of 17% (self reported) and 8% (verified) were observed in panels in the program community. In the comparison community, rates of smoking cessation were 7% (self report) and 1.5% (verified). Within the mass media/volunteer work and intensive contact community, no difference was observed in smoking cessation rates among smokers exposed to a community wide program, and those assigned to receive personal counselling.

Popham et.al.⁽²⁸⁾ report the results of a major anti-tobacco campaign carried out in California. Financed by a 25-cent-per-pack State surcharge on cigarettes, the \$28.9 million campaign employed television, radio, billboards and print media. A year after the campaign was conducted, the role of the media in encouraging cessation was assessed via telephone interviews with 417 adult smokers who had quit during the past year. Of the respondents who had quit, 6.7% attributed their decision to quit to the tobacco ads.

A number of the Australian evaluations feature "time series" type research designs, using frequent cross-sectional surveys of self-reported smoking. In 1983 and 1984, a number of Australian states implemented anti-smoking campaigns. These campaigns used prime time television commercials designed to motivate smokers to quit, accompanied by school education programs within individual communities. The long-term effect of television advertising to reduce the prevalence of smoking was assessed through self-reported smoking data collected from 1981 onwards for each city. Researchers obtained prevalence data for the several years prior to the onset of the intervention, as well as for the duration of the campaign. In 1983 and 1986, survey questionnaires for each city contained more details about beliefs, intentions and influences. Random surveys also were conducted approximately two months after the completion of each component of the television campaign to assess recall of commercials. Smoking prevalences were fitted with a statistical model to identify any underlying trend, to assess any immediate impact, and to assess the longer term effect of continuing such

campaigns. During the years before the campaigns there was no observable trend in smoking prevalence. At the beginning of the campaigns there was an immediate drop in smoking prevalence of more than 2% among men and women. No post campaign trend was observed⁽¹⁷⁾.

Similarly, the Victorian Smoking and Health Program produces annual reports monitoring their various campaigns (eg, Young Women's Campaign; non-English background campaigns; and a "Name Your Poison" Information Campaign). For example, the Young Women's Campaign used a combination of national media (Quit for Life); radio advertisements, promotions based upon health beauty and fitness in collaboration with major fashion and clothing chains and "advertorials" relating to smoking/passive smoking and its relationship to health, beauty, fitness, appearance, pregnancy, etc. The media tracking campaign included such elements as aided and unaided recall, reactions to various commercials (believability; likeability; credibility, etc.); smoking knowledge, attitudes, beliefs, intentions, barriers to quitting and (self-reported) behaviours⁽²¹⁾. The dominant methodological approach used to assess these campaigns is a series of monitoring, or "media tracking" studies conducted using telephone and/or at home interviews. While These monitoring activities may not constitute evaluation activities in the pure sense (eg, no control groups, etc.) they would constitute a key component of an overall evaluation strategy.

Media Campaigns And Smoking Prevention:

Increasingly, media campaigns have begun to shift their focus from providing information to the general public about smoking and health⁽³⁰⁻³⁷⁾ to specific attempts to discourage the onset of smoking, particularly among adolescents and youth^(27,38-42). In this section we describe a number of key evaluation studies in this area, and a number of the major media tracking/monitoring studies that have been undertaken to date.

A study by Bauman et.al⁽³⁸⁾ examined mass media campaigns designed to prevent smoking by adolescents. The campaigns featured radio and television messages on expected consequences of smoking, and a component to stimulate personal encouragement of peers not to smoke. Six Standard Metropolitan Statistical Areas in the Southwestern United States received one of three treatment conditions, and four served as controls. The first campaign used eight 30-second radio messages; the second supplemented the radio messages with financial incentives (sweepstakes); and the third included a television broadcast of the sweepstakes offer. Adolescents and mothers provided pre-test and post-test data in their homes. Adolescents completed a lengthy (one hour) questionnaire, and provided alveolar breath and saliva samples to measure tobacco use. The mothers also completed a shorter (25 minute) questionnaire and provided breath samples. The radio campaign had a modest influence on adolescent's expected consequences of smoking and peer approval of smoking. The more expensive campaign had no greater effect. None of the campaigns had a significant effect upon smoking behaviour.

Flynn^(39,40) examined the ability of mass media interventions to enhance the efficacy of school smoking prevention programs. For four years, students in one pair of communities received media interventions and school programs. Students in a matched pair of communities received only the school program. The

combined cohort of 5458 students was surveyed at baseline in Grades 4,5 and 6, and was followed up annually for 4 years, and again two years after the interventions were completed, when students were in Grades 10 through 12. For cigarettes per week the reduction associated with the media component was 41% (2.6 vs 4.4); for smoking cigarettes yesterday the reduction was 34%; for smoking during the past week, the reduction was 35%. No effects were observed for substance abuse behaviours not targeted by the interventions. The effects of the intervention were found to persist two years after termination of the intervention.

The Minnesota-Wisconsin Adolescent Tobacco Use Research Project⁽⁴²⁾ employed a nested cross-sectional design to compare outcomes in Minnesota and Wisconsin, so as to assess the impact of the Minnesota tobacco control initiative. The mass media component of this \$10 million initiative targeted adolescents using television and radio ads as well as billboards. Monitoring data indicated that at least 90% of the target population was exposed to these ads during the first year, and that on average, they were seen about 50 times per person per year. Approximately 3600 9th graders were surveyed each year in each state (in school), using a self-administered questionnaire and an expired air test for carbon monoxide. The questionnaire included such topics as exposure to anti-smoking messages (aided/unaided recall); smoking related knowledge, attitudes, beliefs, intentions and behaviours. The campaign dramatically increased exposure to anti-smoking messages, but had little effect upon smoking-related beliefs or behaviours.

Proposition 99 in California raised the state excise tax on tobacco by 25 cents a package and used that revenue to discourage tobacco use. A \$28.9 million media campaign and local prevention efforts emerged in 1990 and 1991. The goal of the 15 month Tobacco Education Media Campaign (TEMC) is to help reduce tobacco consumption particularly by adolescents and youth. In fact over 60% of the budget is devoted to this age group. The media campaign combines radio, television, out-of-home advertising and print media. The evaluation studied school aged youth and adult smokers with regards to five different indicators: campaign awareness; reported tobacco use; smokers' intentions to quit; nonsmokers intentions to start; and attitudes regarding smoking. Subjects for the fourth wave of monitoring were 10,711 students in grades four through twelve in various representative school districts as well as adults from specific ethnic subgroups. Smoking and non-smoking students completed four page questionnaires; adults completed 10 minute telephone interviews. Researchers made comparisons between wave 1 and wave 4 data, and comparisons between wave 4 exposed and non-exposed respondents. Results indicated a significant increase in campaign awareness, but little influence on attitudes, intentions or actual (reported) behaviours⁽²⁷⁾.

Campaigns Undertaken As Part Of Larger Initiatives:

Nearly all tobacco mass media campaigns tend to be undertaken in combination with other community based initiatives. These related initiatives may include legislation, policy initiatives, school or workplace based programs, community programming and support services to name but a few. In fact, most of the major anti-tobacco undertakings now tend to adopt a "systems-based" approach, combining a variety of prevention and cessation activities, delivered to a broad range of target groups or population segments through a variety of community

settings (eg, schools, workplaces, health care settings, neighbourhoods, etc.). Some of the better known multi-component campaigns are the California program described earlier⁽²⁷⁾; the ASSIST program (American Stop Smoking Intervention Study for Cancer Prevention)⁽⁴³⁾; the COMMIT program (Community Intervention Trial to Reduce Heavy Smoking) and the Smoking, Tobacco and Cancer program^(44,45).

Another major set of interventions combines mass media anti-tobacco programming as part of a broader, more comprehensive package of health promotion activities. Examples of these comprehensive initiatives are the many "Healthy Lifestyles" projects undertaken in various countries and, more recently, the many major heart health projects and intervention trials⁽⁴⁶⁻⁵⁴⁾. These broad-based initiatives tend to be distinguished by two major features. First, they are among the most thoroughly evaluated projects ever undertaken in this area; secondly, they present a whole series of specific problems relating to the evaluation of specific components such as media campaigns. For example, despite very substantial budgets for evaluation of the major U.S. heart health demonstration projects, Mittlemark⁽⁵⁴⁾ concludes as follows: "The question most often asked of the exemplar programs is "How much did a specific component (eg, media campaign) contribute to the overall effectiveness of the program?" Unfortunately, the answer is always the same, "We don't know." The reason is simple: Community programs assume a synergy among components, with the whole being greater than the sum of the parts. To study synergistic effects many combinations of components would have to be tested against one another. Component analysis in programs with just a few components is difficult and expensive at best; in programs with dozens of components, and hundreds of combinations, component analysis is impossible. Further, tests of components within community programs are utterly contaminated by the larger intervention; we have no way of knowing if outcomes would be similar under different circumstances. So, the decision about which components to adopt cannot be based upon relative component effectiveness. Appropriate criteria for component selection (and evaluation) include: affordability, manageability, compatibility with existing programs; level of community interest, potential for institutionalization, and effectiveness in repeated tests in multiple settings.

The Welsh Heart Program (Heartbeat Wales) combines a variety of evaluation activities at four distinct levels: 1) Problem definition - clarification of the specific inputs, activities and projects which in combination form the Welsh Heart Health Program; 2) Outcome evaluation - a set of survey methodologies used to monitor changes in outcomes and in causal processes; 3) process evaluation - which examines the processes of change; and 4) policy evaluation. The set of monitoring instruments, each of which provide information to several components includes: community surveys; clinical surveys; youth surveys; local project monitoring; macro impact studies; consumer panels; social marketing surveys (media tracking; six month awareness and recall surveys), and health promoters surveys. Nutbeam and Catford propose the utilization of dose-effect measurement procedures to assess program effectiveness by correlating differences in intervention intensity within different populations with the effects on various dependent variables (health behaviours; risk factors, etc.)⁽⁴⁷⁾.

A somewhat similar approach has been developed and applied by Vartiainen and colleagues in their analysis of the North Karelia Heart Health intervention⁽⁴⁸⁾.

Predicted changes in ischaemic heart disease mortality were calculated by a logistic regression model using the risk factor levels assessed by cross-sectional population health surveys carried out in 1972, 1977, 1982, 1987 and 1992. These predicted changes were compared with changes in mortality statistics. Their assessment of the data on the risk factors for ischaemic heart disease and mortality suggested that "most of the decline in mortality could be explained by changes in the three main coronary risk factors... [and] that the population health strategy for preventing heart disease in Finland has been successful". These correlational or "receipt of intervention" models could be applied to analysis of results from mass media components of these interventions as well. Similarly, one could conceivably develop mechanisms for assessing "attributable fractions" that link outcomes to levels of interventions. However, this would require a detailed and integrated program monitoring system⁽⁵⁰⁾.

Recent evaluation studies within the major U.S. Heart Health Demonstration Projects reveal the opposite type of dilemma - evidence that despite demonstrated effectiveness of specific components, the overall strategy was not successful⁽⁵¹⁻⁵³⁾. For example, in their analysis of the Minnesota Heart Health program, Luepker et.al. note that "many intervention components proved effective in targeted groups. However, against a background of strong secular trends of increasing health promotion and declining risk factors, the overall effects were modest in size and duration, and generally within chance levels."⁽⁵³⁾

3.4.4 Conclusions

The evaluation literature relating to mass media campaigns divides broadly into three streams: 1) mass media campaigns directed towards encouraging smoking *cessation*; 2) campaigns which focus upon smoking *prevention*; and 3) campaigns undertaken as part of a broad, comprehensive *multi-component* (eg, ASSIST) or *multi-issue* (eg, Heart Health/ Healthy Lifestyles) intervention. [Note: A fourth stream of media activity that is newly emergent, particularly in California is "media advocacy".] Characteristically, each of these three streams of activity are characterized by very different objectives, methods and approaches, intended outcomes (impacts and effects) and evaluation methodologies/issues.

Our analysis has examined a series of evaluation design and measurement issues, and examples of campaigns in cessation prevention and campaigns as elements of larger initiatives. Many of the large-scale demonstration projects identified in this chapter provide excellent examples of evaluation designs of considerable relevance to the Tobacco Demand Reduction Strategy. A complete review is beyond the scope of this chapter, the focus of which is media interventions. Nevertheless, it is strongly urged that the TDRS undertake such a review, in order to provide a macro-level evaluation as a baseline against which evaluations of individual components or projects may be measured.

A general conclusion can be reached in relation to the evaluation of media campaigns: taken together, the results suggest the importance of integrating media campaign evaluations within the larger context of the macro evaluation framework for the Tobacco Demand Reduction Strategy.

3.4.5 References - Additional Information

1. Flay BR. Mass media and smoking cessation: a critical review. *American Journal of Public Health*, Feb. 1987, Vol 77: 153-160.
2. McKillip J. Evaluation of health promotion media campaigns. In: MT Braverman (Ed.), *Evaluating Health Promotion Programs: New Directions for Program Evaluation*, 1989; No.43: 89-100.
3. Flay BR, McFall S, Burton D, Cook TD, Warnecke RB. Health behaviour changes through television: The roles of de facto and motivated selection processes. *Journal of Health and Social Behaviour*. 1993,34: 322-335.
4. LaPrelle J, Bauman KE, Koch GG. High inter-community variation in adolescent cigarette smoking in a 10-community field experiment. *Evaluation Review*. April 1992, 16(2): 115-130.
5. Roberts DF, Maccoby N. Effects of mass communication. In: G. Lindzey and E. Aronson (Eds.), *Handbook of Social Psychology*. 1985, Vol.2: New York: Random House.
6. Farquhar, JW, Maccoby N, Wood PD, et.al. Community education for cardiovascular health. *Lancet*, 1977, Vol.4: 1192-1195.
7. Brown JD, Bauman KE, Padgett CA. A validity problem in measuring exposure to mass media campaigns. *Health Education Quarterly*, 1990, 17(3): 299-306.
8. Prochaska JO, DiClemente CC, Norcross JC. In search of how people change: Applications to addictive behaviours. *American Psychologist*. 1992, September: 1102-1114.
9. Velicer WF, DiClemente CC, Prochaska JO and Brandenburg N. A decisional balance measure for assessing and predicting smoking status. *Journal of Personality and Social Psychology*. 1985. Vol.48: 1279-1289.
10. Prochaska JO, Velicer WF, DiClemente CC and Fava JS. Measuring processes of change: Applications to the cessation of smoking. *Journal of Consulting and Clinical Psychology*. 1988, Vol.56: 520-528.
11. Prochaska JO and DiClemente CC. Stages and processes of self-change in smoking: Toward an integrative model of change. *Journal of Consulting and Clinical Psychology*. 1983, Vol.5: 390-395.

12. Horn D. and Waingrow S. Some dimensions of a model for smoking behaviour change. *American Journal of Public Health*. 1966. Vol.56: 21-26.
13. DiClemente CC and Prochaska JO. Processes and stages of change: Coping and competence in smoking behaviour change. In: S. Shiffman and TA Wills (Eds.). *Coping and Substance Abuse*. San Diego: Academic Press, 1985: 319-343.
14. DiClemente CC and Prochaska JO. Self-change and therapy change of smoking behaviour: A comparison of processes of change in cessation and maintenance. *Addictive Behaviours*. 1982, Vol.7: 133-142.
15. Prochaska JO and DiClemente CC. Common processes of self change in smoking, weight control and psychological distress. In: S. Shiffman and TA Wills (Eds.). *Coping and Substance Abuse*. San Diego: Academic Press, 1985: 345-363.
16. Prochaska JO, Velicer WF, DiClemente CC. et.al. Patterns of change: Dynamic typology applied to smoking cessation. *Multivariate Behavioral Research*. 1991, Vol.26: 83-107.
17. Pierce JP; Macaskill P; Hill D. Long term effectiveness of mass media led anti-smoking campaigns in Australia. *American Journal of Public Health*, 1990, Vol. 80: 565-569.
18. Egger G, Fitzgerald W, Frape G. et.al. Results of large scale media campaign in Australia: North Coast "Quit for Life" Program. *British Medical Journal*, 1983, 287(6399): 1125-1128.
19. Higham B. *Quit For Life*. Australia: New South Wales Department of Health, 1982.
20. Mullins R. and Borland R. Smoker's responses to the 1990 and 1991 Quit Campaigns: Results from two telephone surveys. In: D. Hill (Ed.). *Quit Evaluation Studies: Vol.6, 1990-91*. Australia: Victorian Smoking and Health Program, August, 1992, 31-52.
21. Mullins R. and Borland R. Evaluation of the 1990 Young Women's Campaign: Results from a telephone survey. In: D. Hill (Ed.). *Quit Evaluation Studies: Vol.6: 1990-91*. Australia: Victorian Smoking and Health Program, August, 1992, 53-66.
22. Korhonen HJ, Niemensivu H, Piha T, Koskela K, Wilo J, Johnson CAP. National TV Smoking Cessation Program and Contest in Finland. *Preventive Medicine*. 1992, 21(1), 74-87.

23. Korhonen HJ, Puska P, Lipand A and Kasmel A. Combining mass media and contest in smoking cessation: An experience from a series of national activities in Finland. *HYGIE*. 1993, 12(1): 15-18.
24. Sogaard AJ and Fonnebo V. Self-reported change in health behaviour after a mass media-based health education campaign. *Scandinavian Journal of Psychology*. 1992, 33(2): 125-134.
25. Nater B, Gutzwiller F, Abelin T et.al. Evolution de la consommation de tabac dans 2 villes d'intervention et 2 villes temoins Suisses. *Revue d'Epidemiologie et de Sante Publique* (Paris), 1985, 33(2), 80-89.
26. McAlister AL, Ramirez AG, Amezcua C, Pully L, Stern MP, Mercado S. Smoking cessation in Texas-Mexico border communities: a quasi-experimental panel study. *American Journal of Health Promotion*. 1992, 6(4): 274-279.
27. Popham WJ, Potter LD, Hetrick MA, Muthen LA. *Evaluating the California 1990-91 Tobacco Education Media Campaign: Executive Report Number 4. Wave 4 Survey Results*. Los Angeles, California, IOX Assessment Associates, September, 1991. (36pg.)
28. Popham WJ, Potter LD, Bal DG, Johnson MD, Duerr JM, Quinn V. Do anti-smoking media campaigns help smokers quit? *Public Health Reports*. 1993, 108(4): 510-513.
29. Gritz ER, Carr CR, and Marcus AC. Unaided smoking cessation. Great American Smokeout and New Year's Day Quitters. *Journal of Psychosocial Oncology*. 1988, 6(3): 217-234.
30. Wewere ME, Ahijevych K, Page JA. Evaluation of a mass media community smoking cessation campaign. *Addictive Behaviours*. 1991, 16(5): 289-294.
31. Erickson AC, McKenna JW, Romano RM. Past lessons and new uses of the mass media in reducing tobacco consumption. *Public Health Reports*. 1990, Vol. 105: 239-244.
32. Wallack L; Sciandra R. Media advocacy and public education in the Community Intervention Trial to Reduce Heavy Smoking (COMMIT). *International Quarterly of Community Health Education*. Special Issue. Vol.11, No.3, 1990-1991, 205-222.
33. Bettinghaus EP. A Mass Media Campaign. Using the mass media in smoking prevention and cessation programs. An introduction to five studies. *Preventive Medicine*, 1988, 17(5): 503-509.
34. Pierce JP, Farkas A, Evans N, Berry C, Choi W, Rosbrook B, Johnson M, Bal DG. *Tobacco Use in California. A Focus on Preventing Uptake in Adolescents*. Sacramento, CA: California Department of Health Services, 1993.
35. U.S.D.H.H.S. Mass media interventions. In: *Smoking, Tobacco and Cancer Program: 1985-1989 Status Report*. U.S. Dept. Health and Human Services, Public Health Service, National Institutes of Health, National Cancer Institute, NIH Publication No. 90-3107.
36. Cramer T. Anti-schoolchild-smoking campaign in Denmark. *Acta Unio Internationalis Contra Carcum*, 1963, 19: 935-936.

37. Research Services of Great Britain. *Report on a Survey to Monitor the Health Education Councils' Adolescent Anti-smoking Campaign*. British Health Education Council, 1976.
38. Bauman KE, LaPrelle J, Brown JD. The influence of three mass media campaigns on variables related to adolescent cigarette smoking: Results of a field experiment. *American Journal of Public Health*. 1991, Vol. 81: 597-604.
39. Flynn BS, Worden JK, Secker-Walker RH. Prevention of cigarette smoking through mass media intervention and school programs. *American Journal of Public Health*, 1992, Vol 82: 827-834.
40. Flynn BS, Worden JK, Secker-Walker RH. Mass media and school interventions for cigarette smoking: Effects two years after completion. *American Journal of Public Health*. 1994, Vol 84: 1148-1150.
41. Moore and Associates, Inc. *1989 Michigan Smoking Prevention Media Campaign: An Evaluation Report*. Lansing, Michigan: Michigan Dept. Public Health, 1990.
42. Murray DM, Prokhorov AV and Harty KC. Effects of a statewide anti-smoking campaign on mass media messages and smoking beliefs. *Preventive Medicine*. 1994, 23(1): 54-60.
43. Novick LF. *American Stop Smoking Intervention Study For Cancer Prevention (ASSIST)*. New York Dept. Health, 1991.
44. U.S.D.H.H.S. *Smoking, Tobacco and Cancer Program (STCP): 1985-1987 Status Report*. U.S.D.H.H.S. Publication No. 90-3107.

45. Wallack L, Sciandra R. Media advocacy and public education in the Community Intervention Trial to Reduce Heavy Smoking (COMMIT). *International Quarterly of Community Health Education*. 1990-1991, 11(3): 205-222.
46. Nissinen A, Puska P. Community control of chronic diseases: a review of cardiovascular programs. *Health Promotion International*, 1989, 4(1): 393-420.
47. Nutbeam D. and Catford J. The Welsh Heart Health Program evaluation strategy: Progress, plans and possibilities. *Health Promotion International*. 1987, 2(1): 5-18.
48. Vartiainen E, Puska P, Pekkanen J et.al. Changes in risk factors explain changes in mortality from ischaemic heart disease in Finland. *British Medical Journal*. 1994, 309: 23-27.
49. Shea S and Basch E A Review of Five Major Community-Based Cardiovascular Disease Prevention Programs. Part II. *American Journal of Health Promotion*. 1990, 4(4): 203-213.
50. Flora JA Lefebvre RC Murray DM et.al. A community education monitoring system: methods from the Stanford Five-City Project, the Minnesota Heart Health Program and the Pawtucket Heart Health Program. *Health Promotion Research: Theory and Practice*. 1993, 8(1): 81-95.
51. Fortmann SP, Barr Taylor C, Flora JA and Jatulis DE. Changes in adult cigarette smoking prevalence after five years of community health education: the Stanford Five-City project. *American Journal of Epidemiology*. 1993, 137(1): 82-96.
52. Perry CL, Kelder SH, Murray DM, Klepp KI. Community-wide smoking prevention: Long-term outcomes of the Minnesota Heart Health Program. *American Journal of Public Health*. 1992, 82: 1210-1216.
53. Luepker RV, Murray DM, Jacobs DR et.al. Community education for cardiovascular disease prevention: Risk factor changes in the Minnesota heart health program. *American Journal of Public Health*. 1994, 84(9): 1383-1392.
54. Mittlemark MB, Hunt MK, Heath GW and Schmid TL. Realistic outcomes: Lessons from community-based research and demonstration programs for the prevention of cardiovascular diseases. *Journal of Public Health Policy*. 1993, 437-462.

3.5 Tobacco Prices And Their Effect On The Use Of Tobacco

3.5.1 Introduction

The purpose of this section of the report is to identify and assess methodologies used to evaluate the effectiveness of tobacco prices and taxation as an intervention made by public authorities in countries other than Canada to reduce the use of tobacco.

Price changes of goods and services have been known to affect the individual and collective health consumption and production decisions. Prices have been used, both intentionally and inadvertently, to affect health behaviours. Pricing and taxation of tobacco and other products, such as alcohol, have long been used as means of generating revenue for governments. Historically, cigarette excise taxes have not been levied primarily to discourage smoking, rather, these taxes have been viewed as a convenient and effective revenue-raising device. Nevertheless, there is abundant evidence that cigarette price has a significant impact on consumption (Warner, 1981).

A variety of tax policies work to encourage or discourage certain consumption behaviours relevant to health. Prominent among these are excise taxes (Warner and Murt, 1984). Excise taxes are imposed on certain goods, such as tobacco products, liquor, and gasoline, primarily as revenue-raising devices. An excise tax on cigarettes is clearly a disincentive to consumption.

Elasticity

It is desirable to have a precise estimate of the relationship between a change in price and a change in consumption, that is, the price elasticity of demand, in order to evaluate the effect of tobacco taxation. Elasticity is a concept in economics which has been used to measure the effect of price changes on the demand and supply of goods. For a description of demand and supply schedules, price and elasticity, see Appendix A. Basically, price elasticity measures the percentage change in consumption given a one per cent change in price. For example, a price elasticity of demand for cigarettes of -0.4 would imply that a 10% increase in the real price (after adjusting for inflation) would lead to a 4% decline in consumption. By and large, tobacco behaves like other commodities; that is, demand goes down as price goes up. This responsiveness is termed either elastic or inelastic, depending on the degree to which demand is influenced by price. For example, demand is said to be price elastic if a 10% change in price produces more than a 10% change in consumption; inelastic if consumption changes by less than 10%. In other words, elastic consumption is defined by elasticities less than -1.0, and inelastic consumption by elasticities greater than -1.0. Perfect inelasticity (insensitivity to cost) is defined by an elasticity of 0.0 (Hursh, 1980). The consumption of cigarettes is in general not very price elastic, and is more elastic among young people than among adults, because young people have less money. Therefore, tax increases will especially affect the smoking behaviour of young people.

3.5.2 General Strategies

1. Model Types

There are several model types used to estimate the price elasticity for the demand for tobacco:

First, there is a "static" model where quantity changes are explained by contemporaneous changes in price and income. It assesses the impact of price and income on the demand for tobacco by econometric methods (Maddala, 1981). This is the simplest model. The most commonly applied model in the demand analysis of tobacco products relates the per capita consumption of tobacco product (Q) to a number of explanatory variables which may include real price of tobacco product (P), real price of other tobacco products as possible substitutes (P_i), real disposable income per capita (Y), and other variables known to affect consumption of tobacco products such as advertising, smoking publicity and health education (X). It is widely acknowledged that tobacco causes psychological and physiological dependency. Smoking easily becomes a habit which is difficult to abandon. The basic model does not take into account the direct effects of habit formation (i.e. tobacco addiction) on the demand. Indirectly its effects come up in the low values of the elasticity estimates; the demand for tobacco is not very responsive to changes in its price. It is not possible, however, to isolate the direct effect of habit formation in the basic model.

Second, there is a partial adjustment (or habit persistence) model (McGuinness and Cowling, 1975) for tobacco addiction. The most simplest way to introduce habit formation into the static model is to postulate a partial adjustment scheme assuming that actual level of demand adjusts gradually to the desired level. It is a dynamic model where shocks to the factors which influence demand take time to be fully incorporated into the quantity consumed. It includes past consumption (per capita consumption of tobacco product in the previous period) as a means of capturing the addictive nature of tobacco consumption. Testing the statistical significance of the coefficient of the past consumption variable (lagged variable) is equal to testing the hypothesis that the consumption of tobacco adjusts gradually to changes in prices and income. When the coefficient of the lagged consumption is zero, the model is reduced back to the basic static model, which implies the demand adjusts immediately (within the period) to changes in prices and income. Given the habitual nature of tobacco consumption, a partial adjustment model would be an obvious choice for estimating the demand for tobacco.

Third, there is the Houthakker-Taylor model (or state adjustment model) (Houthakker and Taylor, 1970), a dynamic model which assumes that consumers of addictive products are influenced by an unobservable psychological requirement that a certain quantity (or stock) must be consumed. The model also formulates a method by which the psychological requirement changes through time. According to this model, a smoker's current demand for tobacco products depends not only on prices and income, but also on the psychological stock of smoking habits that a consumer has built up over his/her smoking life. The stock of habits describes a smoker's current smoking decisions as the result of past behaviour. Given tastes and income, a smoker's current consumption is affected by the psychological stock of smoking habits; the more he/she smoked in the past, the more he/she smokes currently. The state adjustment model is very similar to the partial adjustment model. The only difference between the two is the approach taken to explain how demand changes over time: the partial adjustment model assumes that there is a "desired" level of consumption that is a weighted average of current and past consumption, while the state adjustment model postulates that at any given time the habitual nature of smoking can be captured by a "stock" or quantity that needs to be consumed to maintain the habit, and this "habit stock" changes through time based on current consumption and the depreciated value of the previous period's habit stock. There are relatively few reported analyses of cigarette demand based on the state adjustment model. This is somewhat surprising given the attractive theory underlying it. Perhaps the lack of interest is due to estimation problems encountered in its application that, if not adequately addressed, can lead to errors. For example, some estimations of the demand equations may yield results that are not supportive of the underlying theory; the implied coefficient on the smoking habit stock may be negative while the theory would suggest a positive value. The coefficient on the smoking habits stock measures the strength of the addiction. In this context, only a positive value would make any theoretical sense. In other situations, if the estimation does not adequately take into account the restriction in the state adjustment model which requires that there be only one rate of depreciation in the "habit" stock, errors such as a positive elasticity estimate may be obtained.

Fourth, there is the "rational addiction" model (Becker and Murphy, 1988) that allows future health consequences, as well as one's past consumption history, to influence current consumption. The rational addiction model (Becker and Murphy, 1988) maintains that there are important intertemporal linkages in cigarette consumption that ought to be accounted for when estimating demand. Unlike the partial adjustment model, where the dynamics are limited to the influence of past consumption on current consumption, the rational addiction model allows for the possibility that individuals consider the future when making their current consumption decisions. This is theoretically possible if one allows current utility to depend on current consumption and past consumption. This effectively links consumption decisions made in the past, present and future. One important implication is that, since future prices affect future consumption directly, they indirectly affect current consumption through the interaction of consumption through time.

Fifth, there is the addiction asymmetry model (Scitovsky, 1978). While the partial adjustment, habit stock, and rational addiction models can be interpreted to incorporate the habit forming nature of smoking through their dynamic structure, they suffer from the same shortcoming. The consumers' responses to changes in prices and income are assumed to be symmetric, i.e. the strength of their reactions to declining and increasing prices are equal, despite the habits that they have developed. However, a smoker tends to acquire habits to consume more easily than to abandon them. It can therefore be argued that the asymmetric rather than the symmetric response to changes in prices may well be a

typical feature of consumption of goods. Young (1983) has suggested a model which provides direct estimates of the degree of asymmetry to price and income changes. Young (1983) took as a basis of this model the assumption that the existence of some degree of addiction may result in consumers responding asymmetrically to changes in price and income changes. Consumers may have a tendency to acquire a habit more easily at times of low prices or high income and they may be reluctant to abandon it when prices rise or income falls. Therefore, the expected response to a price rise would be smaller than to a price fall in the same amount.

Sixth, there is the reinforcer interaction model (DeGrandpre et al, 1994) which is a model in behavioral economics (Hursh 1980, 1984). This model has been proposed to study drug dependence based on the understanding of choice behavior. A basic principle of behavioral economics is the demand law, which states that consumption decreases as prices increases (Allison, 1983). The effects of price on the consumption of a reinforcer can be quantified in terms of own-price elasticity which measures the proportional change in consumption across different price conditions. The interaction between different reinforcers can be quantified as a continuum in terms of cross-price elasticity. At one end of this continuum, reinforcers are substitutes; as the price of one reinforcer (e.g. preferred brand of cigarette) increases and its consumption decreases, consumption of a second reinforcer (the substitute, e.g. generic brand of cigarette) increases. At the other end of this continuum, reinforcers are complements; as the price of one reinforcer (e.g. cigarettes) increases and its consumption decreases, the consumption of a second reinforcer (the complement, e.g. coffee) also decreases (DeGrandpre et al, 1994). Cross price elasticities greater than 0.0 (positive slopes) indicates a substitute, while cross price elasticities less than 0.0 (negative slope) indicate complements.

2. Data Types

In general, there are 4 types of data used for estimating the demand for tobacco (Godfrey, 1989):

First, there are time-series data, where the demand for tobacco is estimated over a given time frame based on observed changes in aggregate quantity, price and income. This is the most common type of data (Fujii, 1980; Schneider et al, 1981; Young, 1983; Bishop and Yoo, 1985; Porter, 1986).

Second, there are pooled time-series data, where time-series data are obtained from different areas or regions, in an effort to expand the number of observations (ACIR, 1985; Baltagi and Levin, 1986; Becker et al, 1988; Chaloupka and Saffer, 1988).

Third, there are sample surveys (cross-sectional data) (Lewit et al, 1981; Lewit and Coate, 1982; Mullahy, 1985; Chaloupka, 1988). Several surveys of individual or household behaviour, including data on smoking and drinking habits, are available, and analyses of such data can supplement time-series analyses. From cross-sectional data, the demand is estimated based on individual expenditure data for a given year, and tobacco expenditures are compared across varying demographic characteristics and income levels with the location of the household determining the observed price. In other words, the price variation is derived from the fact that the price of tobacco products differs from one area to another.

Fourth, there are experimental laboratory data (Bickel et al, 1992; DeGrandpre et al, 1994). A small number of subjects, e.g. 6 (DeGrandpre et al, 1994), 7 (DeGrandpre et al, 1992) or 8 (Bickel et al, 1992), are asked to participate in controlled laboratory sessions in which the conditions can be varied and the behavioral choices of the subjects studied.

3. Functional Forms

There are also several functional forms (linear, logarithmic, or some other mathematical transformation) available for data analysis. The logarithmic form, especially the doubly logarithmic form ($\ln Q = a + b \ln P + c \ln Y + dX$), is preferred by many economists both on theoretical and practical grounds (Godfrey, 1989). The advantage of the logarithmic model is that its parameters have a simple interpretation. Estimated parameters indicate directly the values of price and income elasticities.

4. Method of Estimation

Various estimation methods have been used. These methods include ridge regression (Fujii, 1980; Young, 1983), instrumental variables (Schneider et al, 1981; Mullahy, 1985; Baltagi and Levin, 1986; Chaloupka, 1988; Becker et al, 1988), ordinary least-squares (Lewit et al, 1981; Lewit and Coate, 1982; ACIR, 1985), two-stage least-squares (Porter, 1986), three-stage least-squares (Bishop and Yoo), and two-step endogenous law model (Chaloupka and Saffer, 1988).

The empirical results, especially on price elasticities, vary markedly from one model type to another. The results also differ further depending upon the type of data used, the choice of functional forms, and the choice of method of estimation.

3.5.3 Studies - Outcome Measures

The studies reviewed in this section - see Appendix B for study summaries provide a useful survey of the various types of data and models which can be used to estimate the demand for tobacco products when price changes. The estimated initial effect of a change in price on consumption for tobacco products (short-run price elasticity of demand) and the estimated final effect (long-run price elasticity) in various countries are given in Table 1.

Results shown in Table 1 suggest the tremendous potential of cigarette excise taxes as an incentive to improve health behaviour and to cut down on the number of smokers.

However, the approach of using elasticity to estimate the effectiveness of using pricing and taxation to reduce tobacco use has a number of limitations:

1. Elasticity attempts to estimate the effect of price changes on the consumption. While the price of tobacco is an important influence on consumption, there are many other economic and social factors which also affect demand. These include the real price of substitutes (i.e. fine-cut versus cigarettes, since cigarettes and fine-cut tobacco together make up virtually all of the demand for tobacco products), population, education, social class, cigarette advertising, anti-smoking publicity, health consequences, smoking bans, degree of addiction, and taste changes. While changes in price cause movements along the demand curve, changes in these other factors lead to shifts in the demand curve. Unfortunately, it is not possible to include all of these factors in a study since some cannot be observed while others cannot be quantified in any concrete way.
2. Elasticity is influenced by changes in availability. For example, the effect of any price increase may be lessened if, at the same time, there is widespread expansion of licensed vendors, unrestrained advertising, and lax enforcement of regulations.
3. The estimation of elasticity is complicated by the fact that price-induced demand changes begin at the product-type level. Since cigarettes are cheaper when purchased by the carton than by the pack, price increases might simply cause a smoker to switch to buying cigarettes by the carton without having any impact on overall consumption.
4. Elasticity should be estimated separately for different types of tobacco products (e.g. cigarettes, fine-cut tobacco - which is also referred to as manufactured tobacco and is used for roll-your-own cigarettes, cigars, pipe tobacco, chewing tobacco, snuff and plug), since elasticity varies by type of tobacco product. It is difficult to estimate the demand for each type of tobacco product.
5. Elasticity should be estimated separately for different brands and sizes. Even a product like cigarettes can be broken down by brand name and by size. Packages may contain 5, 15, 20 or 25 cigarettes. Cigarettes can be bought by the pack or by the carton (containing 200 cigarettes). Furthermore, cigarettes come in three common sizes - regular, king or 100's. It would be virtually impossible to try to estimate the demand for each brand and size of tobacco products.
6. Elasticity estimates should be estimated separately for different age group (teenagers and adults). Individuals usually begin to smoke early in life. For example, surveys conducted in the United States found that over 75% of adult smokers began to smoke before 21 years of age (U.S. General Accounting Office, 1989). Consequently, there is a good chance that if someone has not started smoking by their mid-20's they never will. Furthermore, individuals who are already addicted to tobacco find it hard to quit.

Consequently, preventing people from starting to smoke has a long-term effect on the total consumption of tobacco products. The question of whether tobacco price increases affect younger people more than adults has not been addressed in many studies. In the United States, there have been four major publications which address this question. Three of these publications (Lewit and Coate, 1981; Lewit et al, 1981; Lewit and Coate, 1982) concluded that younger people are more price sensitive than adults, especially in the context of the decision of whether or not to smoke. However, the study by Wassermen et al (1991) concluded that there were no significant differences in the elasticity of demand between teenagers and adults, since the differences were explained by smoking restrictions such as smoking bans in public places.

7. Elasticity should be estimated separately for different time periods since price elasticity of demand for tax-paid cigarettes changes through time. For example, Laugesen and Meads (1991b) estimated elasticities for each year from 1960 to 1986 for 22 OECD countries. Price elasticity was found to be constant throughout the period, but income elasticity was found to have a decreasing trend over the years.

Methodologies for the estimation of elasticity may encounter problems and difficulties. These may arise during data definition for the dependent variable:

1. The ideal dependent variable for estimating price elasticity would be the actual amount of tobacco consumed. However, such data are available only through surveys, and these results are not available on a consistent basis over a sufficiently long period of time. Furthermore, the health consequences and the negative image associated with smoking provide an incentive to under-report the true level of consumption.

2. An alternative variable to be used as the dependent variable would be the volume of retail sales of tobacco products, and theoretically, imports and domestic shipments should be added to obtain the total volume of sales. However, even if these figures are available with reasonable accuracy, they do not provide a contemporaneously accurate proxy for consumption. Both imports and domestic shipments take time to reach the final consumer and if the time lag associated with imports is different from that for domestic shipments then it would be statistically improper to add the two. For countries with imports representing a small percentage of the total market, it could be argued that excluding imports from the analysis should not pose a significant problem, and the dependent variable in the model could be the quantity of tobacco produced and shipped domestically.

3. The total quantity of tobacco sold in a country is influenced by population. For example, an increase in tobacco consumption may be due to growth in the population of smoking age rather than individuals smoking more. A solution is to estimate the demand for tobacco on a "per-capita" basis using the population age 15 and over. Unfortunately, the use of per-capita consumption may not be ideal since it fails to take into account changes in the demographic make-up of the total population, e.g. age, sex. At any point in time, the proportion of the population who smoke is different across age and sex cohorts (groups). Therefore, a change in the age and sex distribution of the population could, by itself, influence the total amount of tobacco consumed. To account for this, an alternative approach is to divide the aggregate quantity consumed by a weighted population figure where the number of people in each cohort is weighted by a fixed proportion of people who smoke, usually at the beginning of the study period. A second alternative approach would be to explicitly capture demographic characteristics in the demand equation. However, this approach would entail additional parameters and reduce the degrees of freedom, and result in a less powerful statistical model.

Problems and difficulties in the estimation of elasticity may be encountered during the data definition for the explanatory variables:

1. Any model which tries to assess the effect of price changes on demand through time must include a measure of price. Price changes must be estimated by taking into consideration inflation so that the percentage price changes must be that over and above inflation. Through time, inflation reduces the real value of a dollar so that fewer goods and services can be acquired with that dollar. To account for the influence of inflation, values may be expressed in real terms by adjusting for changes in prices relative to a base year. Price elasticities are generally defined with respect to real rather than nominal prices. That is, if the nominal price increased by 10% and the rate of inflation was 10%, then the real price of the product remains unchanged. It is generally assumed that this would not affect the demand for the product.

2. Real price, even after adjusted for inflation, may not be a good independent variable because it should be expressed in relation to pay, especially when an evaluation study uses international data. Godfrey et al (1986a, 1986b) suggested using "number of minutes of work needed to purchase a pack of cigarette" as a measure of price.

3. Besides price, another important explanatory variable is income. One possible measure of income would be personal disposable income. However, it does not reflect the true discretionary income of an individual since it ignores such mandatory expenditures such as rent and interest on personal loans. Furthermore, disposable income includes monies earned by non-residents. Finally, interests and dividend payments contain elements that are related solely to inflation. A way to get round these problems is to have personal disposable income adjusted to account for these factors, then deflated by the total inflation figures, and transformed to a per-capita basis using the population 15 and over. However, the assumptions and techniques used to adjust may create further problems.

4. The demand for any product is also affected by the price of substitutes. The most obvious form of substitution is between cigarettes and fine-cut tobacco. Another form of substitution is between legal and illegal tobacco products. As was reported by Baltagi and Levin (1986), the market for illegal tobacco products has an important and significant effect. Tobacco smuggling generally implies that a tobacco product has been illicitly imported into a country. These importations can be made by professional smugglers or by ordinary individuals. These black market tobacco products are also substitutes for their legal counterparts. Unfortunately, one cannot observe the price for illicit tobacco products. Nevertheless, it is still possible to try to capture the influence of smuggling through the use of a neighbouring state price effect as suggested by Baltagi and Levin (1986).

5. Other forms of substitution may be related to legal importations. For example, there are legal ways to increase one's consumption of tax-free cigarettes by taking advantage of the rule regarding duty-free importations. Smokers could spend more time out of the country (to increase the amount of tax-free cigarettes that can be brought in) or encourage friends to bring them cigarettes on their return from a foreign trip. It is difficult to account for these legal imports as no accurate figures on personal duty-free importations are available.

6. There are other factors which could influence the demand for cigarettes or fine-cut tobacco. These factors include advertising by the tobacco industry, anti-smoking publicity, and other socio-economic factors. However, these factors are difficult to quantify with any precision, making it difficult to include them in the empirical analysis. Specifically, reliable time series data are not available which would capture the effects of such factors as tobacco advertisements and anti-smoking publicity. However, time trends can be introduced in an attempt to capture a decline in smoking which might be attributable to anti-smoking policy. In addition, one could try using dummy variables to test the importance of certain events such as smoking bans.

Problems in estimating elasticity may also be found during the choice of model formulation:

1. There are at least five common models which have been used to estimate price elasticity in empirical studies, i.e. the static model, the partial adjustment model, the state adjustment model, the rational addiction model, and the addiction asymmetry model. These five models are derived from a more general model, the error correction model (Hendry et al, 1984), which attempts to capture all of the factors associated with the demand for tobacco products. The use of different models will yield different estimates of elasticity.

2. Even within a certain selected model, the choice of the functional form of the model such as linear, logarithmic, or some other mathematical form will affect the estimates of elasticity.

3. Two questions which have to be answered in the empirical analysis are: What is the most appropriate model and what is the functional form (linear, logarithmic, or some other mathematical transformation)? To answer these questions, the empirical estimation should follow the Hendry approach, i.e. the error correction model is estimated first and, based on the results, the appropriate functional form is assessed. Several methods, e.g. Box-Cox test (Godfrey and Wickens, 1981), and the PE test (MacKinnon et al, 1982, 1983), are available to test for the appropriate functional form. Once the functional form has been established, the appropriate model can be determined using an F-test, by imposing the required restrictions on the error correction model (the unrestricted model) to arrive at the model in question. For example, the state adjustment model imposes one restriction on the error correction model. The partial adjustment model and the static model impose two and three restrictions, respectively, on the error correction model. From the unrestricted sum of squared residuals (error correction model) and the restricted sum of squared residuals, an F test can be performed to select the appropriate models.

Table 1. Price elasticities of the demand for cigarettes estimated by evaluation studies in some countries other than Canada

Evaluation study	Price elasticity
<u>United States of America</u>	
Maier (1955)	-1.08 to -1.48
Lyon and Simon (1968)	-0.51
Laughunn and Lyon (1971)	-0.81
Fujii (1980)	linear, short-run -0.45
	linear, long-run -0.71
	double log, short-run -0.63
	double log, long-run -0.92
Schneider et al (1981)	-1.23
Lewitt et al (1981)	teenager (12-17 yr) -1.44
Lewitt and Coate (1982)	adult (20-74 yr) -0.42
Harris (1982)	adult -0.26
	teenager -1.20
Young (1983)	price increase -0.34
	price decrease -0.49
Warner and Murt (1984)	adult -0.13
	teenager -0.43
Bishoop and Yoo (1985)	-0.45
ACIR (1985)	-0.45
Mullahy (1985)	-0.47
Baltagi and Levin (1986)	own-state -0.22
	neighbouring-state 0.08
Porter (1986)	-0.27
Warner (1986)	-0.35
Becker and Murphy (1988)	short-run -0.35
	long-run -0.75
Chaloupka (1988)	short-run -0.26
	long-run -0.40
Becker et al (1988)	long-run -0.75
Chaloupka and Saffer (1988)	-0.28
Chaloupka (1990, 1991, 1992)	-0.27 to -0.48
Tegene (1991)	no structural change -0.47 to -0.49
	structural change -0.16 to -0.69
Seldon and Boyd (1991)	short-run -0.26
	long-run -0.32

Table 1. (Continued)

Evaluation study	Price elasticity
Flewelling et al (1992)	-0.25 to -0.35
Keeler et al (1993)	short-run -0.30 to -0.50
	long-run -0.50 to -0.60

DeGrandpre et al (1994)

-0.23 to -0.40

United Kingdom

Sumner (1971)

-0.25

Atkinson and Skegg (1973)

-0.25

HM Treasury (1980)

-0.46

Witt and Pass (1981)

-0.32

Jones and Posnett (1988)

log-log, no addiction

-0.31

log-log, addiction

-0.18

Jones (1989)

overall

-0.56

consumption

-0.37

participation

-0.19

Bosanquet and Trigg (1990)

-0.50

Borren and Sutton (1992)

men, 5 social classes

-0.31 to -0.89

women, 5 social classes

-0.45 to -1.04

Finland

Rimpela and Kuuluvainen (1976)

-0.36

Sehm (1977)

-0.33

Valtonen (1982)

-0.35

Perukinen (1989)

price increase

-0.49

price decrease

-0.94

Greece

Stavrinos (1987)

short-run

-0.10

long-run

-0.14

Table 1. (Continued)

Evaluation study		Price elasticity
<u>Netherlands</u>		
Van Dal (1976)		-0.49 to -0.67
<u>New Zealand</u>		
Laugesen and Meads (1991a)	long-run	-0.41
<u>OECD Countries</u>		
Laugesen and Meads (1991b)		-0.19
<u>Papua New Guinea</u>		
Chapman and Richardson (1990)	cigarette, excise elasticity	-0.71
	non-cigarette, excise elasticity	-0.50
<u>Spain</u>		
Valdes (1993)	short-run	-0.60
	long-run	-0.69

3.5.4 Conclusions

In summary, basic concepts on demand, supply, price and elasticity are reviewed in this chapter. The methodologies (model types) and data types for estimating price elasticity are described and compared. A comprehensive review of the use of various methodologies (model types) and data types to evaluate the price elasticity for the demand for tobacco products in various countries other than Canada is given. Lastly, the effectiveness of using price elasticity, and a number of weaknesses and difficulties in the estimation procedures are pointed out, and a number of ways for improvement are suggested.

Furthermore, results shown in Table 1 suggest the tremendous potential of cigarette excise taxes as an incentive to improve health behaviour and to cut down on the number of smokers. We have also identified a number of limitations to using elasticity to estimate the effectiveness of using pricing and taxation to reduce tobacco use. We have also identified problems and difficulties in using a variety of methodologies for the estimation of elasticity. Additional problems may be encountered during the definition for exploratory variables. Finally problems in estimating elasticity may also be found during the choice of model formulation. This analysis is offered as an overview of elements to consider in estimating the impact of price increases on tobacco use.

3.5.5 References/Additional Information

1. ACIR. Cigarette tax evasion: A second look. Washington, DC: Advisory Commission on Intergovernmental Relations, 1985.
2. Allison J. Behavioral economics. New York: Praeger Publishers, 1983.
3. Atkinson AB, Gomulka J, Stern NH. Household expenditure on tobacco 1970-1980: Evidence from the Family Expenditure Survey, LSE/ESRC Programme on taxation, incentives and the distribution of income, Discussion paper No. 57.
4. Atkinson AB, Skegg JL. Anti-smoking publicity and the demand for tobacco in the U.K. *Manchest Schl* 1973;41:265-282.
5. Baltagi BH. Pooling: An experimental study of alternative testing and estimation procedures in a two-way error component model. *J Econometrics* 1981;17:21-49.
6. Becker G, Grossman M, Murphy K. An empirical analysis of cigarette addiction. Working paper. Department of Economics, University of Chicago, 1988.
7. Becker G, Grossman M, Murphy K. An empirical analysis of cigarette addiction. Working paper No. 3322. Cambridge, MA: National Bureau of Economic Research, 1990.
8. Becker G, Murphy K. A theory of rational addiction. *J Political Economy* 1988;96:675-700.
9. Bickel WK, Hughes JR, DeGrandpre JR, Higgins ST, Rizutto P. Behavioral economics of drug self-administration. IV. Own-price and cross-price analysis of concurrent access to coffee and cigarettes. *Psychopharmacology* 1992;107:211-216.
10. Bishop JA, Yoo JH. Health care, excise taxes and advertising ban in the cigarette demand and supply. *Southern Econ J* 1986;52:402-411.
11. Borren P, Sutton M. Are increases in cigarette taxation regressive? *Health Econ* 1992;1:245-253.
12. Bosanquet N, Trigg A. A smokefree Europe in the year 2000: Wishful thinking or realistic strategy? London: St. Mary's RHBNC Health Policy Unit, Discussion Paper No. 4, 1990. (Cited in Bosanquet N, Trigg A. Smoking and economic incentives in Europe. *Br J Addiction* 1991;86:627-630.)
13. Chaloupka FJ. An economic analysis of addictive behavior: The case of cigarette smoking. PhD Dissertation (City University of New York), 1988.
14. Chaloupka F. Rational addictive behaviour and cigarette smoking. Working paper No. 3268. Cambridge, MA: National Bureau of Economic Research, 1990.
15. Chaloupka F. Rational addictive behaviour and cigarette smoking. *J Political Economy* 1991;99:722-742.
16. Chaloupka F. Clean indoor air laws, addiction and cigarette smoking. *Applied*

Economics 1992;24:193-205.

17. Chaloupka F, Saffer H. The demand for cigarettes and restriction on smoking in the workplace. NBER Working Paper No. 2663.

18. Chapman S, Richardson J. Tobacco excise and declining tobacco consumption: The case of Papua New Guinea. *AJPH* 1990;80:537-540.

19. Cooley TF, Prescott EC. An adaptive regression model. *Int Economic Review* 1973;14:364-371.

20. Cooley TF, Prescott EC. Estimation in the presence of stochastic parameter variation. *Econometrica* 1976;44:167-183.

21. DeGrandpre RJ, Bickel WK, Higgins ST, Hughes JR. A behavioral economic analysis of concurrently available money and cigarettes. *J Experimental Analyses of Behavior* 1994;61:191-201.

22. DeGrandpre RJ, Bickel WK, Rizvi SAT, Hughes JR. Effects of income on drug choice in humans. *J Experimental Analyses of Behavior* 1992;59:483-500.

23. Flewelling RL, Kenney E, Elder JP, Pierce J, Johnson M, Bal DG. First-year impact of the 1989 California cigarette tax increase on cigarette consumption. *AJPH* 1992;82:867-869.

24. Fujii E. The demand for cigarettes: Further empirical evidence and its implications for public policy. *Applied Economics* 1980;12:479-489.

25. Godfrey C, Hardman G, Powell M. Data note: 1. Alcohol, tobacco and taxation. *Br J Addiction* 1986a;81:143-149.

26. Godfrey C, Hardman G, Maynard A. Data note: 2. Measuring U.K. alcohol and tobacco consumption. *Br J Addiction* 1986b;81:287-293.

27. Godfrey C. Factors influencing the consumption of alcohol and tobacco: The use and abuse of economic models. *Br J Addiction* 1989;84:1123-1138.

28. Godfrey LG, Wickens MR. Testing linear and log-linear regressions for functional forms. *Rev Economic Studies* 1981;48:487-496.

29. Haden K. The demand for cigarettes in Japan. *Amer J Agr Econ* 1990;72:446-450.

30. Harris JE. Increasing the federal excise tax on cigarettes. *J Health Econ* 1982;1:117-120.

31. Hausman JA, Taylor WE. Panel data and unobservable individual effects. *Econometrica* 1981;49:1377-1398.

32. Hendry DF, Pagan AR, Sargan JD. Chapter 18. Dynamic specification. In: Griliches Z, Intriligator MD (eds). *Handbook of Economics*. Vol II. Amsterdam: North Holland, 1984.

33. Hoerl A, Kennard R. Ridge regression: Biased estimation for nonorthogonal problems. *Technometrics* 1970;12:55-67.
34. Houthakker HS, Taylor LD. Consumer demand in the United States: Analysis and projections. Cambridge, Massachusetts: Harvard University Press, 1970.
35. Hursh SR. Economic aspects for the analyses of behavior. *J Experimental Analyses of Behavior* 1980;34:219-238.
36. Hursh SR. Behavioral economics. *J Experimental Analyses of Behavior* 1984;42:435-452.
37. Jones A. The U.K. demand for cigarettes 1954-1986: A double-hurdle approach. *J Health Econ* 1989;8:133-141.
38. Jones A, Posnett J. The revenue and welfare effects of cigarette taxes. *Appl Econ* 1988;20:1223-1232.
39. Keeler TE, Hu T-W, Barnett PG, Manning WG. Taxation, regulation, and addiction: A demand function for cigarettes based on time series evidence. *J Health Econ* 1993;12:1-18.
40. Laugesen M, Meads C. Advertising, price, income and publicity effects on weekly cigarette sales in New Zealand supermarkets. *Br J Addiction* 1991a;86:83-89.
41. Laugesen M, Meads C. Tobacco advertising restrictions, price, income and tobacco consumption in OECD countries, 1960-1986. *Br J Addiction* 1991b;86:1434-1354.
42. Laughhunn DJ, Lyon HL. The feasibility of tax induced price increases as a deterrent to cigarette consumption. *J Business Admin* 1971;3:27-35.
43. Lewit EM, Coate D. The potential for using excise taxes to reduce smoking. Working paper No. 764. National Bureau of Economic Research, 1981.
44. Lewit EM, Coate D, Grossman M. The effects of government regulation on teenage smoking. *J Law Econ* 1981;24:545-569.
45. Lewit EM, Coate D. The potential for using excise taxes to reduce smoking. *J Health Econ* 1982;1:121-145.
46. Lyon HL, Simon JL. Price elasticity of the demand for cigarettes in the United States. *Am J Agricultural Economics* 1968;50:881-893.
47. Mackinnon JG, White H, Davidson R. Some further results on tests for model specification in the presence of alternative hypotheses. Discussion paper 491, Queen's Institute for Economic Research, Queen's University, 1982.
48. Mackinnon JG, White H, Davidson R. Tests for model specification in the presence of alternative hypotheses, some further results. *J Econometrics* 1983;21:53-70.
49. Maddala GS. *Econometrics*. Tokyo: McGraw-Hill, 1981.

50. Maier FH. Consumer demand for cigarettes estimated from state data. *J Farm Economics* 1955;37:690-704.
51. McGuinness T, Cowling K. Advertising and the aggregate demand for cigarettes. *European Economic Review* 1975;6:311-328.
52. Mullahy J. Cigarette smoking: Habits, health concerns, and heterogenous unobservables in a microeconomic analysis of consumer demand. PhD Dissertation (University of Virginia) 1985.
53. Palda KS. The measurement of cumulative advertising effects. Englewood Cliffs: Prentice-Hall, 1964.
54. Perkurinen M, Valtonen H. Price, policy and consumption of tobacco: The Finnish experience. *Soc Sci Med* 1987;25:875-881.
55. Perkurinen M. The demand for tobacco products in Finland. *Br J Addiction* 1989;84:1183-1192.
56. Peterson DE, Zeger SL, Remington PL, Anderson HA. The effect of state cigarette tax increases on cigarette sales, 1955 to 1988. *Am J Publ Health* 1992;82:94-96.
57. Porter RH. The impact of government policy on the U.S. cigarette industry. In: Ippolito PM, Scheffman DT (eds). *Empirical approaches to consumer protection economics*. Washington, DC: Federal Trade Commission, pp.447-481.
58. Rimpela M, Kuuluainen J. The relation between the price, consumption and the quality of tobacco in Finland. *Sosiaalilaaket Aikakausi* 1976;4:908-927.
59. Samuelson PA, Scott A. *Economics: An introductory analysis*. Toronto: McGraw-Hill, 1968.
60. Schneider L, Klein B, Murphy K. Governmental regulation of cigarette health information. *J Law Econ* 1981;24:575-612.
61. Scitovsky T. Asymmetries in economics. *Scottish J Political Econ* 1978;25:227-237.
62. Sehm M. The price of cigarettes as a determinant of the demand. *Sosiaalilaaket Aikakausi* 1977;3:199-209.
63. Seldon BJ, Boyd R. The stability of cigarettes demand. *Applied Economics* 1991;23:319-326.
64. Sumner MT. The demand for tobacco in the U.K. *Manchest Schl* 1971;39:23-26.
65. U.S. General Accounting Office. Teenage smoking: Higher excise tax should significantly reduce the number of smokers. A report to the Honourable Michael A. Andrews, House of Representatives, 1989.
66. Valdes B. Cigarette consumption in Spain: Empirical evidence and implications for public health policy. *Appl Economics* 1993;25:149-156.

67. Valtonen H. An interpretation of the price policy of tobacco. *Sosiaalilaaket Aikakausi* 1984;21:40-46.
68. Van Dal FWA. Een nieuwe analyse van het Nederlandse tabaksverbruik. *Economisch Statistische Berichten* 1976:524-527.
69. Van Reek J, Hagen H, Adriaanse H, van Iwaarden T. Changes in Dutch tobacco production since World War II: Implications for an anti-smoking policy. *Health Policy* 1989;11:233-238.
70. Warner KE. State legislation on smoking and health: A comparison of two policies. *Policy Sci* 1981;13:139-152
71. Warner KE. Smoking and health implications of a change in the federal cigarette excise tax. *JAMA* 1986;255:1028-1032.
72. Warner KE, Murt HA. Economic incentives for health. *Ann Rev Public Health* 1984;5:107-133.
73. Wasserman J, Manning WG, Newhouse JP, Winkler JD. The effects of excise taxes and regulations on cigarette smoking. *J Health Economics* 1991;10:43-64.
74. Witt SF, Pass CL. The effects of health warnings and advertising on the demand for cigarettes. *Scot J polit Econ* 1981;28:86-91.
75. Young T. The demand for cigarettes: Alternative specifications of Fujii's model. *Appl Economics* 1983;15:203-211.
76. Zellner A, Geisel MS. Analysis of distributed lag models with applications to consumption function estimation. *Econometrica* 1970;38:865-888.

3.6 Non-Price Restrictions

3.6.1 Introduction

A wide scope of interventions are available for consideration within this category. Those include:

Access and Availability/Control of Sales

- Age of purchase
- Unit sales (package size) and number of packages/plain packaging
- Hours of sale
- Licensing of vendors
- Places of sale
- Types of businesses that can sell tobacco products
- Systems of fines for illegal sales, and their application
- System of monitoring for illegal sales
- Tracking of product from production through to wholesale and retail sales to reduce opportunity for contraband sales
- Types of product that can be sold (type of tobacco and form, e.g., loose)
- Display of product in retail places
- Volume of sales of merchants

Promotion of Tobacco Products

- Promotion of sporting/cultural events
- Promotional give-away of product
- Advertising (e.g., media, transportation, sporting/cultural events)
- Restrictions on import (legal and smuggled) of foreign brands and advertising
- Portrayal in movies, plays, television
- Display of product in retail places
- Existence/placement of in-store advertising

Other Policy Initiatives

- Smoking in public places
- Education of retailers
- Existence and placement of signs (in retail and public places)
- Smoking in places of work

As a result of our review of available literature, the most prevalent interventions and readily accessible evaluation information are related to the following four interventions:

- A. Smoking Restrictions in Public Places
- B. Reducing Youth Access to Tobacco
- C. Advertising
- D. Cigarette Warnings

We have chosen an exemplary study for each type of intervention and provide an analysis of the strengths and weaknesses for the evaluation of each approach.

3.6.2 General Strategies

A. Smoking Restrictions in Public Places

The intent of restricting smoking in public places is to reduce environmental tobacco smoke exposure and consequently affect smoking behaviour by encouraging current smokers to quit and discouraging non-smokers from starting. However, laws restricting smoking in public places can protect the public only if they are obeyed. The evaluations of this form of intervention sought to assess compliance with the smoking restrictions in public places. Although it may also be of interest to determine what affect the intervention had on smoking rates in general, no such evaluations were found in the literature.

The studies done which assessed this form of intervention were essentially outcome evaluations. Selected public places were observed prior to smoking restrictions taking effect and several times after with the intent of measuring the degree of compliance with the new smoking laws. In most cases, a public place was considered to be compliant if there was a no-smoking sign posted, and if there was no evidence that any employee or member of the public smoked in that place. However, without continual surveillance, it is difficult to ascertain whether a public place is being compliant with the smoking restrictions. Also, without a control community being observed during the same time frame, it is impossible to attribute any change in behaviour unequivocally to the law. Any improvements may also reflect concurrent changes in social norms about smoking.

B. Reducing Youth Access to Tobacco

Most new smokers are drawn from the ranks of children. Nicotine is an addiction acquired primarily during the teenage years, at a time when they are considered ill equipped to make lifelong decisions. Interventions aimed at reducing the supply of tobacco to adolescents by making it more difficult for them to obtain tobacco are important components of efforts to reduce teenage addiction to tobacco. Youth obtain cigarettes primarily from commercial sources rather than friends or family (Forster et al., 1992). It follows that laws that effectively prevent the purchase of cigarettes by minors could have a major effect on smoking prevalence in this group.

Many studies have been conducted to attempt to assess how effective laws are which prohibit the sale of tobacco products to minors. Most evaluations of this type of intervention take on the form of repeat cross-sectional observations or surveys in study populations at several points in time. They include a baseline study at some point prior to the implementation of the intervention, then follow-up observations at various time intervals after the law has been put in place. The studies are most often formative

evaluations and outcome evaluations. The most common method of evaluating laws prohibiting the sale of cigarettes to minors involve underage assessors attempting to illegally purchase cigarettes from tobacco outlets, both over-the-counter and by a vending machine (eg. convenience stores, gas stations, restaurants etc.). The main outcome measure is the percentage of successful attempts to purchase cigarettes. Depending on the study, other outcomes measures often include the percentage of clerks who asked for identification, for the age of the youth, and who the cigarettes were being bought for.

C. Advertising

Advertising is used both for health promotion to warn people about the negative health effects of smoking, and by tobacco companies which aim to portray their product in a favourable light. They attempt to communicate messages that link use of tobacco products to positive feelings, images, and experiences. No one denies that cigarette advertising increases the advertisers' market share, what is in question however, is the tobacco industry's stance that total cigarette advertising and total cigarette sales are not related. Many studies have been conducted to assess the influence of cigarette advertising and anti-smoking ads on total sales of cigarettes and on smoker initiation rates.

D. Cigarette Warnings

Many governments currently require sellers to put warnings on ads and packages of cigarettes. Sellers are required to rotate among several different warnings (4 in the U.S.), and they may vary from country to country. Despite the enormity of this warning-focused effort to prevent smoking, there is little or no evidence that the warnings are effective (Malouff et al., 1992). The effectiveness of cigarette warnings can be divided into behavioral effectiveness, which is the ability of warnings to prevent people from beginning to smoke or quit smoking, and potential effectiveness, which is the extent to which warning have certain characteristics expected to make behavioral effectiveness more likely. These characteristics might include the extent to which warnings are noticed, read, understood, believed and remembered. All published studies have examined potential effectiveness because behavioral effectiveness, albeit more important, is very difficult to assess.

3.6.3 Studies - Outcome Measures

A. Smoking Restrictions In Public Places

A study done in Cambridge, Mass, sought to evaluate the compliance of a no-smoking ordinance restricting smoking in public places (Rigotti et al., 1993). They prospectively observed 174 retail stores 1 month before, and 3, 11, and 24 months after the law took effect. An employee from each store was also interviewed at 24 months. Retail stores were chosen because they constitute the largest single group of public places for observation. The sample was taken by selecting every 5th store in the Yellow pages, plotting its location on a map, then clustering the stores into 6 geographic areas. Each cluster was assigned a number of stores to be observed that was proportional to its representation in the Yellow pages sample. The stores were selected for observation by a process by which observers began at a preset central location and observed all retail stores in a predetermined pattern until they reached that cluster's total. The stores were observed 4 times. Each visit took place at the same time of day and day of week, by a different set of observers each time. At each visit, the observers recorded the size, location, type of store, the number of employees and customers, the presence and wording of no-smoking signs, the presence of ashtrays, cigarette butts, a smoker, or the odour of

smoke and the observation time. On the last visit, the first employee seen after entering the store was interviewed. They were asked what the store's smoking policy was, what their position was, and how often they saw customers smoking.

This form of evaluation raises several issues and concerns. Although the primary goal was to measure the compliance of stores with the ordinance, this study cannot distinguish between lack of compliance and lack of knowledge about the law. Unless the store manager was the first employee seen when the observers entered the store, they were not interviewed and thus knowledge about the law cannot be separated from compliance. This is related to another issue, the inaccuracy of employee reports. Only one employee was interviewed per store, regardless of their position or how long they had worked there. Therefore, the reliability of the employee reports is uncertain.

The presence of smokers or the odour of smoke was rarely observed, even before the law took effect. This may reflect the social norms proscribing smoking in retail stores to prevent fires. This raises the issue that retail stores may be of limited use when trying to generalize the findings to other types of public places. The external validity of this study is further threatened by the fact that only one community was studied. Different compliance rates could be due to the wording of statutes or to implementation efforts. The low frequency with which smoking was observed in the stores may also be a function of the short observation time in each store. To account for this, other signs were used as evidence of smoking occurring in the store, such as the presence of ashtrays and cigarette butts. The number of cigarette butts observed decreased over time, however this also could possibly be attributed to other factors, such as more frequent cleaning of the store.

Another issue is the loss of stores to follow-up. During the study period, 32 stores were lost to follow-up because they either moved or closed. The validity of the study is further threatened by the variability between observers' recordings of outcomes, especially for subjective data such as odour of cigarette smoke.

B. Reducing Youth Access To Tobacco

A community-level feasibility (or formative) study was conducted in the Minneapolis-St. Paul region to determine what policy changes communities were willing to implement to decrease child access to tobacco and to determine the effect of these policy changes on children's access to cigarettes (Forster et al., 1992). Fourteen young people ages 12-15 were recruited to be confederates in the study. Three or four different youth attempted to buy cigarettes, on separate occasions, in each tobacco outlet in 3 communities. If asked their age, they were instructed to lie and say 18. In other studies however, the assessors were instructed to tell them their real age when asked, which provides slightly different results. After each attempt, the information about the interaction between young person and any employees was recorded, including the gender and approximate age of the salesperson. In other studies, the assessors also recorded the presence and wording of signs which say that it is illegal to sell tobacco to minors.

There are several limitations to studies of this form. First of all, having adolescents illegally purchase cigarettes is a legal and thus an ethical concern. Some studies have got around this by getting permission from the local authorities then confiscating the cigarettes from the youth after they were purchased. In other studies, when clerks were willing to sell the product (indicated by the clerk ringing up the sale or retrieving the cigarettes from the shelf), assessors said they did not have enough money. This

procedure could have produced reactive effects. Many clerks probably realized that these attempts to buy were assessments for a study. Although they could only have made this connection after the attempt was made, the procedure could have reminded the clerk about the campaign to reduce sales. There is a concern related to the comparability of the two cigarette purchasing protocols, however no significant differences in the results have been found.

Another limitation of these studies is that the young people's ability to buy were partly a function of their apparent age or other physical or behavioral characteristics. The differences between the adolescent assessors could potentially have a large effect on the outcome. Adolescents which look older, or have more experience buying cigarettes because they are smokers themselves will likely be the most successful at purchasing cigarettes. Furthermore, different age-sex categories of minors attempting to make the purchase and the seller can also influence the outcome (older female clerks are less likely to sell to younger males and younger male clerks are most likely to sell to older female adolescents). Another possible limitation is that multiple purchase attempts were made at each outlet, which produces a degree of nonindependence.

In the absence of extensive efforts to educate retailers about the specifics of the law, poor compliance could be confused with lack of knowledge. Although this problem is partially eliminated when the appropriate signs are displayed, compliance cannot be measured unless the particulars of the law are made known.

C. Advertising

The effects of advertising are difficult to measure. Several different approaches have been taken to evaluate the influence of advertising. In one study, children who had admitted to trying a cigarette were directly asked why they had decided to try their first cigarette. The researchers believed that if advertising had influenced their decision to try smoking, children would have identified advertising as a factor (Smith, 1989). A less direct approach was conducted in the U.S. This study examined trends in the rates of smoking initiation by adolescents during the 1980's and relates these to cigarette advertising and promotional expenditures during the same time period (Smoking Initiation by Adolescents and Young Adults, U.S., 1980-90). The data was collected using a tobacco use supplement to the Current Population Surveys (CPS), which is a probability sample based on a stratified sampling scheme. The smoking behaviour of the respondents was determined, and using this data, smoking initiation rates were calculated. Data on cigarette advertising and promotional expenditures for the same period were obtained from annual Federal Trade Commission Reports. Finally, the relationship between cigarette advertising and promotional expenditures and smoking initiation rates for adolescents was evaluated.

There are several possible limitations to this study. First of all, the number of younger adolescents who smoke may be under-represented because they are most likely to underreport smoking behaviour in household and telephone surveys. This study cannot attribute any increase in the smoking initiation rates solely to an increase in advertising expenditures. It fails to control for income, price of cigarettes, the introduction of smoking legislation, and many other factors which could influence smoking initiation rates.

A second study done in New Zealand did a better job of controlling for some of the factors which affect cigarette sales. It focused on advertising, price, income and publicity

effects on weekly cigarette sales in supermarkets (Laugesen et al., 1991). Total cigarettes (all brands) sold weekly by a panel of 60 New Zealand supermarkets were monitored electronically for 42 weeks. Real cigarette price, newspaper advertising of all brands, the number of non-shopping days that week and the next, and the number of newspaper news items on smoking issues were the explanatory variables.

There are limitations to this study as well. Potential cigarette buyers likely come into contact with many cigarette advertisements and anti-smoking ads not found in the newspaper. These advertisements may influence an individual's decision to purchase cigarettes that week, yet this influence is unaccounted for in the study. Furthermore, weekly cigarette sales are compared to real advertising expenditures for that week and the number of newspaper news items on smoking issues that week. It is possible that a newspaper article or cigarette advertisement appeared in one week, but the paper may not have been read until what was considered to be the following week (this would be especially true for weekend papers). It is possible as well, that the people who read the newspaper do not purchase their cigarettes from supermarkets, although there is no evidence of this.

D. Cigarette Warnings

Among the published studies on the potential effectiveness of tobacco warnings, there are several different methods of evaluating these health warnings. Both current warnings and new, proposed warnings are assessed, therefore studies serve as both formative and outcome evaluations. The main drawback of these studies is that there is no experimental evidence showing that any type of warning affects cigarette use, regardless of its potential effectiveness.

A study conducted in Colorado used four common methods of evaluating cigarette warning effectiveness, and explored the convergent validity of the four approaches, the potential effectiveness of several warnings (i.e. to what extent they are understood, remembered, and viewed as effective), and the reasons for differences between warnings in potential effectiveness (Malouff et al., 1992). Essentially, four separate studies were conducted using students in grades 7, 8, and 9 from the same school. No student participated in more than one study. In study 1, 112 7th graders were shown the 4 currently required warnings and 4 possible other warnings. They were asked to rate their effectiveness (the ability of the warning to prevent them from smoking in the future), and to answer questions about the meaning of certain words. In study 2, 115 7th graders were asked to recall as much of the 4 current warnings as possible (the words that come after the "Surgeon General's Warning"). Two assessors then independently decided which warnings they identified. A warning was rated as identified if any 1 or 3 concepts for that warning was mentioned. In study 3, 103 8th and 9th graders read the 4 current warnings and the 4 other warnings one day, and tried to recall them the next day. In study 4, 25 7th graders participated in a focus group session in which they discussed the effectiveness of each of the eight warnings.

The biggest concern about studies like this, as it was mentioned above, is that despite any findings regarding the potential effectiveness of cigarette warnings, there is no evidence that cigarette use will be affected - although students claim that a warning will help them avoid smoking, they could be incorrect about that. The ability to generalize from this study may be limited. Only students in grades 7 to 9 were used, which is only a small portion of the population at risk. Furthermore, even if cigarette warnings are aimed at young people in that age range, all the students came from the same school in a small town in Colorado, therefore they may not be representative of children their age.

There are limitations to the recall studies as well. It would seem that the students who smoke or who are in contact with people who smoke would be better able to recall the current cigarette warnings. This is more of a problem with the 24-hour recall study because students who smoke had likely seen the warnings on actual packages of cigarettes during in the interim. Having been made aware of the cigarette warnings, some students may also take notice of cigarette warnings on ads and packages in the interim, which could improve their recall. In study 2, it was sufficient for a student to mention 1 concept in a particular warning for that warning to be rated as identified. In some situations it is possible that students correctly guessed a concept (eg. "disease", or "death") although they had never actually noticed or read the warning in question. The students' knowledge about the effects of smoking could have been acquired from other sources, such as television, newspaper articles, etc., although it would appear in this study that they had noticed, read, and recalled a cigarette warning.

3.6.4 Conclusions

Our analysis of a wide variety of options available for the use of non-price restrictions has identified four areas most commonly utilized and evaluated in the literature.

Smoking Restrictions in public places were evaluated by outcome evaluations utilizing pre and post smoking restriction measures. The lack of continual surveillance and no control community make it difficult to attribute behaviour changes directly to smoking restrictions.

Laws restricting youth access to tobacco have most often been evaluated by repeat cross sectional observations or surveys at several points in time. The studies are most often formative and outcome evaluations.

Cigarette warnings are the fourth intervention area identified in our review. Despite the enormity of this warning focussed effort to prevent smoking, there is little or no evidence that the warnings are effective. All published studies have examined potential effectiveness because behavioral effectiveness is hard to assess.

The effects of advertising restrictions on smoking are difficult to measure. Studies have not been able to attribute any increase in smoking initiation rates solely to an increase in advertising expenditures.

Finally a series of **recommendations are offered for use in evaluating the federal TDRS program.**

1. That the TDRS establish an external scientific review/advisory committee of researchers which represent expertise in a full range of methodologies specializing in

evaluation for Intervention areas identified in this report to advise on all aspects of TDRS monitoring, tracking and evaluation.

2. That evaluation assessment studies be carried out on all intervention activities undertaken under TDRS, and that specific evaluation frameworks be developed for these components. The evaluation methodologies selected for each intervention should reflect the need for and possibility for rigour and precision suitable to each specific situation.
3. That all evaluation strategies relating to the components of TDRS be subject to external review, or at a minimum, subject to review by an evaluation advisory committee.
4. That specific attention be accorded the major threats to external and internal validity (described in this report) that typically are encountered in the evaluation of national intervention programs.
5. That specific attention be accorded the measurement issues of greatest concern to Tobacco Control evaluations.
6. That a multiple-methods research strategy be developed that "triangulates" results relating to the components of TDRS.
7. That specific measurement issues for each of the intervention areas (eg. media, community, school etc.) of exposure to all TDRS components be incorporated into tracking and evaluation studies.
8. That Health Canada consider commissioning additional studies on the outcomes of evaluations. These studies could utilize materials derived from this study and a similar report completed on Canadian evaluation approaches.

3.6.5 References

1. Forster, J.L., Hourigan, M., and McGovern, P. "Availability of Cigarettes to Underage Youth in Three Communities" *Preventive Medicine* 21, pp. 320-328, 1992.
2. Laugesen, M., & Meads, C. "Advertising, Price, Income, and Publicity Effects on Weekly Cigarette Sales in New Zealand Supermarkets" *British Journal of Addiction* 86, pp. 83-89, 1991.
3. Malouff, J., Schutte, N., Frohardt, M., Deming, W., & Mantelli, D., "Preventing Smoking: Evaluationg the Potential Effectiveness of Cigarette Warnings" *The Journal of Psychology*, 126(4), pp.371-383, 1992.
4. Rigotti, N.A., Stoto, M.A., Bierer M.F., Rosen, A., & Schelling, T. "Retail Stores Compliance with a City No-Smoking Law" *American Journal of Public Health*, vol. 83, no. 2. pp. 227-232, 1993.
5. Smith, G. "The Effects of Tobacco Advertising on Children" *British Journal of Addiction* 84, pp.1275-1277, 1989.
6. Smoking Initiation by Adolescents and Young Adults - United States, 1980-1990.

APPENDIX A

Tobacco Prices And Their Effect On The Used Tobacco

A Description of Demand and Supply Schedules,

Price and Elasticity

The quantity of a commodity (or good) people will buy at any one time depends on price; the higher the price charged for an article, the less the quantity of it people will be willing to buy; and, other things being equal, the lower its market price, the more units of it will be demanded. The relationship between price and quantity bought is called the "demand schedule" or "demand curve", which can be represented by a graph of price (y-axis) versus quantity (x-axis). Quantity and price are inversely related, quantity goes up when price goes down. The curve slopes downward, hence a name for this important property: the law of downward-sloping demand - when the price of a commodity is raised, at the same time that all other things are held constant, less of it will be demanded. Or equally, if a greater quantity of a commodity is thrown on the market, then other things being equal, it can be sold at a lower price. There are two reasons for the downward-sloping demand: (1) lowering prices brings in new buyers, (2) reduction of prices may induce some extra purchases by each of the commodity's consumers. Conversely, when the price of a commodity rises, people try to (1) substitute other commodities for it, and/or (2) cut down on the consumption of it.

Various commodities differ in the degree to which quantity bought responds to changes in price. Elasticity of demand is a concept devised to quantify the degree of responsiveness of quantity demanded to changes in market price. When a cut in price raises quantity so much as to increase total revenue (i.e. price times quantity), the demand is elastic, i.e. elasticity of demand is greater than unity. When a cut in price results in an exactly compensating rise in quantity so as to leave total revenue exactly unchanged, it refers to unitary (or unit) elasticity of demand, i.e. elasticity of demand is equal to unity. When a percentage cut in price evokes so small a percentage increase in quantity as to make total revenue fall, the demand is inelastic, i.e. elasticity of demand is less than unity. Price elasticity coefficient (between two different price points on a demand curve) is equal to the per cent that quantity has changed divided by the per cent change in price.

$$\text{Elasticity} = \frac{\text{percentage change in quantity}}{\text{percentage change in price}} = \frac{dQ/Q}{dP/P}$$

For example, if price increases from 2 cents to 4 cents and quantity decreases from 20 to 10, the price change of 2 (dP) is related to the average price of 3 (mean of 2 and 4) (P) which gives a percentage change of 2/3 or 67%, and the quantity change of -10 (dQ) is related to the average quantity of 15 (Q) which gives a percentage change of -10/15 or -67%. Price elasticity is -67%/ 67% = -1 (Samuelson and Scott, 1968).

Elasticity is not the slope of a demand curve: a common misconception is that a steep slope must mean inelastic demand, and a flat slope must mean elastic demand. This is not true, because the slope of the demand curve depends on absolute change in price and quantity, whereas elasticity is seen to depend on percentage changes. At the top of the demand curve, price is high and its percentage change low, and quantity is very low and its percentage change therefore almost infinitely great, which results in a very high elasticity, as estimated by the numerical formula for elasticity coefficient. In fact, most

demand curves start out elastic at high price and end up inelastic at low price, passing through unitary elasticity at an intermediate position where total revenue (price times quantity) is at its maximum. It can be shown that when a demand curve is plotted on double-log paper, it becomes correct to identify slope with elasticity, because double-log paper does measure percentage changes.

In a similar way, a supply schedule or supply curve can be constructed to show the relation between market prices and the amounts that producers are willing to supply. It is an upward-sloping supply curve. The equilibrium market price is where demand and supply curves intersect.

Supply and demand are not ultimate explanations of price. Rather than final answers, they simply represent initial questions. The government can affect price in several ways: (1) A tax of a certain dollar amount per unit of a commodity will lead to a new equilibrium intersection of the supply and demand curves and will be borne more largely by consumers rather than producers to the degree that the demand is inelastic relative to the supply. The more elastic demand and supply curves are, the less the price change and the greater the quantity change. (2) It can do so by affecting supply or demand, or both. For example, government programs for restricting production of a commodity can raise price and income by cutting down on supply. This can be done by destroying or burning a commodity. (3) Prices may be fixed by law. The government sometimes sets by law a maximum price ceiling or a minimum price floor that interfere with the workings of competitive markets. Under such circumstances, supply and demand need not be equal, and rationing, disorder, and black markets may result.

A sales tax (or increase in sales tax) on a commodity will raise its price most and reduce its quantity least when supply and demand curves are most inelastic. When they are elastic, quantity changes much and price changes little. The tax is shifted forward onto the consumer when the demand curve is very inelastic. It is shifted backward onto the producer when the supply curve is relatively the more inelastic. Only with the apparatus of supply and demand can the economist analyze the final effect (or what economists call the "incidence") of various different taxes, e.g. import tariffs, cigarette excises, etc. A common fallacy regarding the effect of a tax on a commodity is that the tax might seem at first sight to be an advance in price to the consumer, but an advance in price will diminish the demand, and a reduced demand will send the price down again. Therefore, it is not certain, after all, that the tax will really raise the price and reduce the demand. The correct concept would be that a tax will raise the price to the consumer and lower the price received by the producer, with the difference going to the government. At a higher price, a smaller quantity will be bought by consumers. This is as it should be, because producers are also supplying a smaller quantity at the lower price which they receive. Thus the amounts willingly bought and sold are in balance where the new supply and demand schedules intersect, and there will be no further change in price.

APPENDIX B

Tobacco Prices And Their Effect On The Use of Tobacco

Study Summaries

United States of America

Fujii (1980)

Fujii (1980) applied the partial adjustment model to estimate a linear demand equation and a double logarithmic specification using aggregate U.S. data from 1929 to 1973. The dependent variable, per capita consumption, was measured using cigarette production data. The explanatory variables included a real cigarette wholesale price index, real per capita disposable personal income, a cigarette advertising expenditure index, and dummy variables to capture the effects of three events related to the health effects of smoking (including the 1953 health report of the U.S. Department of Health, Education and Welfare, the 1964 Surgeon General's report on smoking and health, and the 1968 Federal Communication Commission's Fairness Doctrine which requires that anti-smoking messages be broadcast.) Fujii used the ridge regression estimating technique because of multi-collinearity in the data. Multi-collinearity, which has been a problem in many of the studies, arises when there is significant correlation between the explanatory variables in the estimating equation. If not addressed in the estimating technique, the parameter estimates will be highly sensitive to changes in specification of the sample data. The ridge regression estimating technique circumvents problems resulting from an almost singular matrix due to multi-collinearity by multiplying each diagonal element of the explanatory variable matrix by $(1+k)$, $0 < k < 1$. Starting with a low initial value, k is increased and the demand equation is estimated until the estimated parameters stabilize. It has been demonstrated that ridge regression yields parameter estimates that are biased, but the average error of the estimates is smaller than the average error of the ordinary least squares estimator (Hoerl and Kennard, 1970). The price, income, advertising and lag coefficients all had the correct sign, were statistically significant, and were stable across the two specifications of demand (the linear and double log specifications). The parameters obtained from the estimation of the linear demand equation (evaluated at the sample means) suggested a short-run price elasticity of -0.48 and a long-run price elasticity of -0.71. When the double log specification is used, the price elasticity of demand was found to be -0.63 in the short run, and -0.92 in the long run.

Lewitt et al (1981)

In an American study, Lewitt et al (1981) applied the static model to data comprised of 6,738 teenagers aged 12 to 17 in the U.S. from 1966 to 1970. They measured smoking both in terms of participation (whether the youth is a current smoker, yes=1, no=0), and quantity consumed (number of packages of cigarettes smoked daily). These dependent variables were regressed on an extensive list of explanatory variables. The estimation technique was ordinary least squares. Those variables reported to affect demand significantly were the real cigarette price index and the number of anti-smoking messages aired. Family and youth characteristics were included in the regressions, but the effects of these were statistically non-significant. Family characteristics captured were real family income, parents' schooling, absence of father from the household, whether the mother works. Youth characteristics included age, sex, race, student status, the number of hours worked per week during the school year, whether the youth works during school

vacations, whether an allowance was earned, region of residence, and size of place of residence. The quantity-smoked regression and the smoking-participation regression both yielded an estimated price elasticity of -1.44 indicating that teenage demand is highly sensitive to price (a 10% price increase implies a 14% quantity decrease among teenagers). Their results demonstrated that cigarette demand by youths is more sensitive than it is for the adult population, due to the habit-forming nature of smoking. Other studies based on aggregate data generally find a much weaker sensitivity to price shocks since the total smoking population consists largely of adult smokers (18 and over). Price increases lead to a decline in youth participation and prompt some adult smokers to quit. The remaining smokers continue their habit largely unaffected by the change. As a result, the measured aggregate elasticity of demand is quite inelastic. The implication is that changes in teenage smoking induced by government policy can have a significant and lasting impact on aggregate consumption in the long run.

Lewitt and Coate (1982)

Lewitt and Coate (1982), using the static model, estimated that the overall price elasticity of demand for cigarettes (a measure of the responsiveness of demand to price changes) is -0.42, meaning that a 10% increase in cigarette price will decrease total cigarette consumption by 4%. Most of this decrease appears to reflect decisions to quit smoking or not to start, rather than decreases in daily consumption by continuing smokers. The authors concluded that "price has its greatest effect on young males and it operates primarily on the decision to smoke rather than via adjustments in the quantity of cigarettes smoked".

Harris (1982)

Harris (1982) estimated that a doubling of the U.S. federal excise tax on cigarettes from 8 cents to 16 cents per pack would increase retail cigarette prices by 12%. Using the results from Lewitt and Coate (1981, 1982), and Lewitt et al (1981), that price elasticities are -0.26 and -1.20 for adults and teenagers, respectively, he estimated that this should result in a 3% decline in the number of adult smokers (or some 1.5 million people), and a 15% decline among teenagers (700,000 teens).

Young (1983)

Young (1983) made alternative specifications of Fujii's model (partial adjustment) to estimate the demand for cigarettes. Young assumed an asymmetric response and applied a "ratchet" model to test this. This model contains two price variables, i.e. price declines and price increases. Using the same explanatory variables and linear demand equation as Fujii, with modifications to allow for asymmetric price and income responses, and the same ridge regression estimating technique, Young's results showed that the short-run price elasticity of demand for cigarettes was -0.34 for a price increase and -0.49 for a price decrease. The estimated elasticities were short-run and calculated at the sample mean. Using a modified F-test, Young rejected the restriction implied by Fujii's model which is that the parameter for a price increase is equal to the negative of the parameter for a price reduction, and claimed support for his hypothesis. Young noted that his results would suggest that taxation policy would seem to be less powerful as a demand deterrent than is suggested by Fujii's elasticity figures.

Warner (1986)

Warner (1986) estimated that an 8-cent decrease in the federal cigarette excise tax could induce up to 1 million young persons, ages 12 to 25 years, to smoke, when without the tax decrease they would not. Conversely, an 8- to 16-cent tax increase would encourage from 1 to 2 million young persons and 800,000 to 1.5 million adults to quit smoking or

not to start. Therefore, intentionally or inadvertently, the federal cigarette excise tax is a powerful tool in public health policy.

Baltagi and Levin (1986)

Baltagi and Levin (1986) estimated a dynamic demand for cigarettes using the partial adjustment model based on pooled time-series of cross-section data of 46 states over the period 1963 to 1980. Their work is important because of its attempt to incorporate smuggling effects into the demand equation. They found a significant bootlegging effect due to the variation in tax rates across states, i.e. consumers in some states might be tempted to buy from lower tax states so the consumption per capita is over-reported in the lower tax states, and under-reported in the higher tax states. Therefore, price elasticity estimates based on such figures would be biased if the presence of smuggling were ignored. The approach taken was to incorporate a "substitute" price variable, measured as the minimum real price of cigarettes in the neighbouring states. In their study, the cigarette demand equation (C) was modelled based on the real price of cigarettes within state (P), real disposal income per capita (Y), per capita index of advertising (A), and the health dummy variables (H). The bootlegging effect was modeled by the use of a neighbouring state price effect (P_n) which acted as a substitute price. In addition, they assumed that cigarette consumption was governed by a partial adjustment model (McGuinness and Cowling, 1975), and that advertising enters with an infinite geometric lag (Palda, 1964). Estimation and test of hypotheses were based on an error component model (Baltagi, 1981). Hausman-Taylor's (1981) estimation procedure was applied to account for the fact that advertising is invariant across states. Zellner-Geisel's (1970) estimation procedure was applied to distinguish between lagged effects due to advertising and those due to habit persistence. They found that for each state, own price elasticity was -0.22 and neighbouring price elasticity was 0.08. Their results suggested a small but statistically significant smuggling effect on demand. This means that a tax increase across the board raising real prices in all states by 10% will reduce consumption per capita of cigarettes by only 1.4%. Income and advertising were found to be insignificant determinants of demand.

Becker and Murphy (1988)

The rational addiction model was applied to U.S. cigarette sales data from 1955 to 1985 (Becker and Murphy, 1988; Becker et al, 1990). Their results showed that a fall in price in the current period increases consumption in that period which then increases consumption in the following period because of the addictive nature of smoking. Both future and past price changes are found to negatively affect current consumption. Their parameter estimates indicated that the elasticity of demand with respect to current price is -0.35 in the short run if the price stock is anticipated, and -0.30 if it is unanticipated. The long-run price elasticity was estimated to be -0.75.

Chaloupka (1990, 1991, 1992)

Applying the rational addiction model to national U.S. micro data for a sample of 28,000 individuals of all ages for years 1976 to 1980 and using the instrumental variables estimation method, Chaloupka (1990, 1991, 1992) obtained long-run price elasticity estimates ranging from -0.27 to -0.48.

Tegene (1991)

In this study by Tegene (1991), a demand equation was used to describe the relation of annual per capita consumption of cigarettes per person over 14 (Q) and three explanatory variables: relative cigarette price index (1967=100) (P), real per capita disposable personal income in 1972 dollars (Y), and corporate advertising expenditure index

(1967=100) defined in per capita terms (A). The partial adjustment (or habit persistence) model was assumed. Assuming no structural change, the estimated price elasticities for the years 1953 to 1985 ranged from -0.471 to -0.489; with structural change, they ranged from -0.164 to -0.693. It was found that cigarette demand was inelastic with respect to price and income, and advertising was statistically insignificant for the most part. Furthermore, both income and price elasticities declined over time while advertising elasticity tended to rise. Four time periods where structural change had occurred were identified. The health scare reports and the Fairness Doctrine Act had a significant impact on per capita cigarette consumption.

Flewelling et al (1992)

Flewelling et al (1992) employed a time series design to evaluate the impact of the 1989 California cigarette tax increase on cigarette consumption in California. Adult per capita consumption data trend analysis indicated a sharp drop in California cigarette consumption coincident with the tax increase. Time series regression analyses supported this observation and suggested that a 5% to 7% decline in consumption was attributable to the tax increase. The estimated impact of the 1989 tax increase represents a price elasticity of between -0.25 and -0.35.

Peterson et al (1992)

There was a substantial drop in national cigarette sales in the U.S. in 1983 following the increase of the federal cigarette excise tax from 8 to 16 cents on January 1, 1983. A number of economists commented on this as a cause-effect relationship. In order to study this relationship, Peterson et al (1992) conducted a study to see whether state cigarette tax increases, of which there have been 248 during the years 1955 through 1988, have had similar effects. They assessed changes in cigarette consumption following state cigarette tax increases and quantified the amount of the change in cigarette consumption by the size of tax increase. All tax and price data were converted to constant 1983 dollars. Calculating the differences in sale (cigarette consumption) for each of the states for each year of the study period yielded 1536 observations of changes in cigarette consumption. The robust variance method of Zeger and Liang (1986) was used to estimate the means and standard errors of the categorized changes in cigarette consumption. Results indicated that cigarette consumption declined by an average of 3.0 packs per capita in the 248 observations with a tax increase. In contrast, cigarette consumption rose by 0.6 packs per capita in the 1288 observations with no tax increase. Regressing the size of tax increase on the change in cigarette consumption, it was found that for each 1-cent increase in cigarette taxes, sales fell by 0.75 packs per capita (about 0.6%). This study also looked at the effect of enactment of state clean indoor air acts but failed to evaluate the effects of other socio-economic variables on cigarette consumption.

Keeler et al (1993)

Keeler et al (1993) analyzed the effects of prices, taxes, income and anti-smoking regulations on the consumption of cigarettes in California, based on monthly time-series data from 1980-1990. Two models, with and without rational addiction, were specified. They found that consumers behave consistently with the model of rational addiction. Their results showed a price elasticity of demand for cigarettes in the short run of -0.3 to -0.5, and -0.5 to -0.6 in the long run.

DeGrandpre et al (1994)

DeGrandpre et al (1994) applied the behavioral economic conceptualization of own-price and cross-price elasticities to study the effects of response requirement on, and interaction between, concurrently available money and cigarettes, based on experimental data. Their study examined the interaction between cigarettes and money and whether they function as substitutes, complements, or independents in a laboratory context. This is the first study which investigates the interaction between a drug reinforcer (cigarettes) and nondrug reinforcer (money) in a behavioral economic framework. Two female and four male cigarette smokers, ranging from 18 to 48 years of age who reported smoking from 20 to 25 cigarettes per day, participated in the study. Subjects participated in from 7 to 17 4-hr laboratory sessions in which money and puffs on a cigarette were concurrently available according to fixed-ratio schedules of reinforcement. Once stable responding was obtained with both reinforcers available at fixed ratio 100, the response requirement for one reinforcer was systematically varied, while the other reinforcer remained

scheduled at fix ratio 100. Own-price and cross-price elasticities of demand were estimated using the equations derived from Allison (1983). Results indicated that consumption decreased as price (fix ratio) increased. Own-price elasticities for the puff manipulation were estimated to vary from -0.23 to -0.40. Manipulating the price of one reinforcer increased consumption of the other reinforcer in only 3 of the 6 subjects. Little or no interaction was found for the remaining subjects.

Other U.S. studies

A number of other U.S. studies also provided estimates on price elasticities. Maier (1955) arrived at a high range of price elasticities (-1.08 to -1.48) using a cross-section study of state data. Lyon and Simon (1968), using a quasi-experimental approach, computed price elasticities using state sales data before and after a tax change and found a price elasticity of -0.51 with a 95% confidence interval of -0.35 to -0.71. Laughhunn and Lyon (1971) supported cigarette taxation as an effective policy tool in reducing cigarette consumption. They estimated a price elasticity of -0.81 and found that this demand for cigarettes is stable over time. Schneider et al (1981), using time series aggregate data from 1930-1970, provided a price elasticity estimate of -1.23. Warner and Murt (1984) estimated that a substantial tax hike, of 30 cents over the 1982 rate of 8 cents, could decrease adult consumption by 15% and teenage consumption by more than 50%. Bishop and Yoo (1985), using times series aggregate data from 1954-1980, provided a price elasticity estimate of -0.45. The Advisory Commission on Intergovernmental relations (ACIR) (1985), using pooled time series of state cross sections from 1981-1983, estimated a price elasticity of -0.45. Mullahy (1985) used probit analysis and found a price elasticity of -0.47. Porter (1986) conducted two-stage least squares analysis on time series aggregate data from 1947-1982 and found a price elasticity of -0.27. Chaloupka (1988) found a short-run elasticity of -0.26 and a long-run elasticity of -0.40. Becker et al (1988) found a long-run elasticity of -0.75. Chaloupka and Saffer (1988) used a two step endogenous law model on the pooled time series of state cross sections from 1975-1985 and found a price elasticity of -0.28. Seldon and Boyd (1991) applied the Cooley-Prescott technique (Cooley and Prescott, 1973, 1976) to data from the U.S. from 1953-1984 and found a short-run price elasticity of -0.26 and a long-run elasticity of -0.32.

United Kingdom

Godfrey et al (1986a, 1986b)

In the U.K., Godfrey et al (1986a, 1986b) measured tobacco consumption by volume and expenditure for 1960, 1965, 1970, and 1975 to 1984. It was a descriptive study based on tabulations on cigarette excise duty, price indices, work hours vs. purchasing power, tobacco revenue, trends in consumption trade and employment, and calculated effects of budget strategies. They found that real expenditures for tobacco fell by 23% from 1965 to 1984, and total expenditure for tobacco fell from 6.7% to 3.4%. The retail price index for tobacco kept pace with other prices during the period. After accounting for pay and price increases, fewer minutes of work were needed to purchase a pack of 20 cigarettes in 1984 than in 1960 (22.3 vs 35.7 minutes). Tobacco consumption by weight decreased from 1960 to 1984 (24.8%), and so did consumption by number (10.7%). Sales in numbers of cigarettes increased by 19.6% from 1960 to 1975, but sales by weight decreased by 11.2%. Sales of cigars and cigarillos increased by 400% from 1960 to 1975 and by 286% over the whole period. Between 1975 and 1984, tobacco consumption decreased, whichever measure was used. The average number of cigarettes smoked by women aged 16 years or over increased from 1972 to 1984, but there was a turning point in 1980 when consumption declined. Men's consumption remained relatively constant, but men in older age groups increased their cigarette consumption.

Godfrey and Maynard (1988)

Godfrey and Maynard (1988), utilizing the same approach as Godfrey et al (1986a, 1986b) to study the effects of price, income, and tax options on tobacco consumption, found that during the previous 10 years, tobacco consumption in the U.K. had steadily declined, cigarette prices had fluctuated, the purchasing power of smokers had increased, and the taxation rate for tobacco products had risen. Over 70% of all tobacco sales went into the British treasury. The value of tobacco tax revenues was 10% lower in 1986 than at their peak in 1965, and tobacco revenue was becoming a smaller proportion of total tax receipts. They pointed out that a consistent increase in tobacco taxation would reduce consumption and harm to health but would also reduce employment. As consumers change their buying habits, increased employment in other sectors of the economy may compensate for some job losses in the tobacco industry.

Jones and Posnett (1988)

Jones and Posnett (1988) examined the extent to which available evidence in the U.K. supports the case for taxation as a means of reducing smoking. The parameters used in this study were derived from an econometric analysis of the demand for cigarettes in the U.K. based on annual data for the period 1956-1982. The estimation was based on a double-hurdle perspective, first proposed by Atkinson et al (1984), which argues that participation and consumption decisions are a result of two different processes. The participation equation and the per smoker demand equation were therefore estimated separately. Price elasticities were estimated to be -0.31 for a log-log model without addiction, and -0.18 for a log-log model with addiction. The price elasticity of -0.46, which had been used in official calculations in the U.K. (HM Treasury, 1980), was also included in the estimation of change in consumption. It was estimated that with a price elasticity of -0.18, a 10p increase in duty fully passed on to the consumer would reduce consumption by 1.8%, or by 2.5 cigarettes per week for a smoker on 20-a-day. On the higher official estimate of price elasticity of -0.46, the reduction in consumption would be around 4.5%, or 6 cigarettes per week. In this study, the basic unit of consumption was assumed to be a pack of 20 king-size cigarettes, initial price was assumed to be 100p, duty per unit was 60p, and change in cigarette duty was 10p. As the authors noted, this clearly overlooked the variety of brands, packaging, and pricing in the cigarette market.

Jones (1989)

Jones (1989) used a double-hurdle perspective which suggests that it is crucial to distinguish between per capita and per smoker consumption when estimating the demand for cigarettes, in an empirical analysis of aggregate time series for the U.K. demand for cigarettes based on 1954-1986 data. A two-equation model was set up which identified consumption and participation as two separate decisions, and which was based on Deaton and Muellbauer's Almost Ideal Demand System. The model was estimated with annual data on U.K. cigarette expenditure and participation rates, highlighting the differing influences of prices, income and health scares on the participation rate and on smokers' demand for cigarettes. The price elasticity of per capita demand was estimated to be -0.562, which could be decomposed into an effect on consumption and an effect on participation, with an estimated elasticity of demand of -0.370 and of participation of -0.192. In other words a 10% increase in the price of cigarettes would be expected to reduce per capita demand by around 5.6%, and of this 1.9% would be a result in participation while 3.7% would be due to the fall in smokers' demand. At the 1986 U.K. participation rate of 33%, the 1.9% reduction in participation translates to a drop of 0.6% to 32.4%. Therefore the results suggested that, in the U.K., taxation may not be a very effective weapon for a government concerned about reducing the prevalence of smoking in the population as a whole. However, Jones pointed out that price rises may be an

effective tool if the objective is to prevent cash-constrained teenagers from acquiring the habit. Jones also cautioned that, in his study, cigarette consumption obtained from household survey data may have problems with under-reporting and non-response.

Bosanquet and Trigg (1990)

Bosanquet and Trigg (1990) estimated that an increase of 10% in the real price of cigarettes was associated with a fall in consumption of 5%.

Borren and Sutton (1992)

Borren and Sutton (1992) used a log-linear single equation model which assumes that the price and income elasticities of demand have remained constant over the period 1961-1987 in the U.K. The demand equations were estimated using ordinary least squares. For men, price elasticities were found to be -0.69, -0.48, -0.84, -0.89, and -0.31 for social classes 1 (highest income group) to 5 (lowest income group), respectively. There was no obvious pattern of increasing price elasticity across the classes. For women, price elasticities were estimated to be -1.04, -0.93, -0.65, -0.85 and -0.45 for social classes 1 to 5, respectively. Although there were no obvious patterns between social classes, elasticities for women were considerably higher than those of their male counterparts.

Other U.K. Studies

Price elasticities were also estimated in a number of other U.K. studies to be -0.25 (Sumner, 1971), -0.25 (Atkinson and Skegg, 1973), and -0.32 (Witt and Pass, 1981).

Finland

Pekurinen and Valtonen (1987)

Pekurinen and Valtonen (1987) summarised the findings of numerous Finnish studies on the estimation of price elasticities for tobacco products and found surprisingly similar results. For example, price elasticities were estimated to be -0.36 (Rimpela and Kuuluvainen, 1976), -0.33 (Sehm, 1977), and -0.35 (Valtonen, 1982). These studies have used time-series data for different sub-periods from 1952 to 1984 applying somewhat different specifications of the demand functions. Results of these studies can be summarised as follows: cigarette price elasticity is about -0.35, pipe tobacco price elasticity is about -0.60, cigar price elasticity is about -2.00, demand for pipe tobacco also depends on the price of cigarettes and the cross-price elasticity is about +2.00.

Pekurinen (1989)

Four time-series models were applied by Pekurinen (1989) to estimate the demand for tobacco products in Finland: a basic demand model, and three habit formation (addiction) models (partial adjustment model, habit stock model, and addiction asymmetry model). Different models were found to be appropriate for different tobacco products. The addiction asymmetry model appeared to be the best for cigarettes and pipe tobacco. The basic demand model was appropriate for cigars and all tobacco products. The empirical results did not lend support to the partial adjustment model nor to the habit stock model. The price of cigarettes was found to be the single most important determinant of the demand of tobacco products. In Finland, the price of tobacco is controlled by the government by means of an excise tax levied on tobacco products. The demand for cigarettes was twice as sensitive to falling prices (elasticity = -0.94) as to rising prices (elasticity = -0.49).

Greece

Stavrinos (1987)

By setting up a demand function of cigarettes which specified the dependent variable as the number of per capita demanded cigarettes, and explanatory variables as the per capita disposable personal income, the price of cigarettes, advertising expenditure, and information provision and health education on the risks of cigarette smoking, and using ridge regression on data from 1960 to 1982, Stavrinos (1987) found that in Greece a 10% increase in the price of cigarettes would lower consumption only by 1% in the short-run and by 1.4% in the long-run.

Japan

Haden (1990)

Haden (1990) used a two-stage budgeting procedure to estimate demand elasticities for cigarettes in Japan, based on data for the years spanning 1964-1983. Data collected included prices and quantities of imported cigarettes, retail prices of cigarettes, tax and tariff rates on cigarettes, and an index of consumer food prices. All prices were converted to yen per pack and were deflated by the Japanese CPI, and consumption was in pack per person per year. Income was measured by gross national product per person in yen. In this study, cigarette consumption in Japan was disaggregated into Japanese cigarettes, cigarettes imported from the U.S, and cigarettes from the rest-of-world. The first stage of the budgeting procedure was hypothesized to have the share of per capita income expended on all cigarettes in period t (M_t) regressing on the expenditure share in the period $t-1$ (M_{t-1}), a geometric price index (P_t^*), an index of prices of goods other than cigarettes (Z_t), per capita income in period t (Y_t), and a time trend to reflect changes in health concerns and tastes and preferences (YR). The system of equations for the second stage were estimated using a static AIDS (Almost Ideal Demand System) model, with homogeneity and symmetry imposed. The static model was estimated using iterative regressions which produces results equivalent to maximum likelihood. The error terms from the estimated equations contained no statistical evidence of serial correlation, suggesting that a dynamic formulation in the second stage was unnecessary. Results indicated that the own-price elasticity for U.S. cigarettes was much less than that for Japanese cigarettes or rest-of-world cigarettes. The own price-elasticity on U.S. cigarettes sold in Japan was fairly similar to values which have been estimated for the own-price elasticity in the U.S. No habit effects were found at the level of expenditure shares between types of cigarettes, implying that consumers adjust their expenditure shares completely in one time period to changes in prices and income. Japanese consumers have not developed a habit formation for U.S. cigarettes, as compared with other types; neither have they developed habits specifically for Japanese cigarettes.

Netherlands

Van Reek et al (1989)

Van Reek et al (1989) quoted the results of Van Dal (1976) who calculated the price elasticity of tobacco production (change in production in % per 1% consumer price increase), based on annual production, price and income figures in the Netherlands in the period 1957-1974. Van Dal found price elasticities of -0.67 in 1957 and -0.49 in 1974. The authors pointed out three validity problems of using production figures: border traffic, loss of non-smoked tobacco products, and rate of circulation. Therefore production figures are not completely identical to real consumption.

New Zealand

Laugesen and Meads (1991a)

Total cigarettes (all brands) sold weekly in New Zealand supermarkets were monitored electronically for 42 weeks. Real cigarette price, newspaper advertising, and the number of newspaper news items on smoking issues were inversely associated with cigarette sales. Newspapers, by doubling news coverage of smoking issues or by banning cigarette advertisements, can lower cigarette consumption as much as a 10% price increase can. Price appears to be the most powerful variable which governments can influence. The long-run price elasticity was found to be -0.41.

OECD Countries

Laugesen and Meads (1991b)

Laugesen and Meads (1991b) studied factors affecting tobacco consumption per adult in 22 countries of the Organization for Economic Cooperation and Development (OECD) between 1960 and 1986 using pooled cross-section time-series analysis. The resulting log-linear model was estimated using Generalized Least Squares. Lower consumption levels were associated with higher real tobacco prices, more tobacco advertising restrictions, increased female labour force participation, lower per capita real income, and with a smaller fraction of tobacco consumed as manufactured cigarettes. The model explained 99.5% of the variance in the average annual level of tobacco consumption across these countries. Real price elasticity of tobacco products consumption per adult across 22 OECD countries was estimated to be -0.189, meaning a decrease of 0.189% in tobacco consumption per adult of a one percent change at the mean real price of tobacco. Across the OECD, if in 1986 all governments had raised tobacco product prices relative to income to Irish levels, and had banned all tobacco promotion, tobacco products consumption per adult would have fallen by 40% in that year.

Papua New Guinea

Chapman and Richardson (1990)

Chapman and Richardson (1990) reported the first price elasticity estimates for the demand for tobacco products for a developing country, based on the volume of cigarettes and non-cigarette tobacco consumed and the excise revenue obtained from the government statistical office of Papua New Guinea for the years 1973-1986. Data therefore permitted calculation of excise elasticity rather than price elasticity. Excise elasticities of demand for cigarettes and non-cigarette tobacco were found to be -0.71 and -0.50, respectively. This means that a 10% increase in cigarette and non-cigarette tobacco excise is associated with an estimated 7.1% fall in the demand for cigarettes and a 5% fall in the demand for non-cigarette tobacco. As excise (tax) elasticities would normally be significantly smaller than price elasticities, these results imply that the consumption of cigarettes and tobacco in Papua New Guinea is more responsive to prices than in the United States and other Western countries.

Spain

Valdes (1993)

Valdes (1993) used the partial adjustment (habit-persistence) model to analyse the behaviour of the demand for cigarettes in Spain over the period from 1964 to 1988. The demand function specified that the quantity of cigarettes that society is willing to buy in period t (Q) depends upon that period's price of cigarettes (P), prices of other consumer goods (O), total disposable income of society (Y), society's taste for cigarettes (T), and other factors such as the existence of legislation restricting smoking in public places (H). Price elasticities of demand were found to be -0.60 and -0.69 for the short- and long-run, respectively.

APPENDIX C

Principal Investigators

Dr. Thomas Abernathy is a Principal Investigator of the Ontario Tobacco Research Unit. Dr. Abernathy is Manager of Community Information Services in the City of Toronto Department of Public Health. His interest in tobacco studies involves fundamental research, its translation into health promotion programming, and program evaluation. Since 1987, as principal investigator, he has conducted an evaluation of the PAL (Peer Assisted Learning) Smoking Prevention Program designed by Health and Welfare Canada for primary-school adolescents. This study, which has involved following the smoking history of nearly 10,000 individual students over a span of five years, is one of the few comprehensive longitudinal evaluations of program efficacy in existence. In 1988, he undertook a survey of nearly 5,000 businesses in the province of Alberta regarding their experience with the design and implementation of a workplace smoking policy, the findings of which have influenced the development of government policy in the area. In 1991 he designed, and carried out, an evaluation of the "Compliance for Kids" program. This community-based tobacco reduction campaign provided new insights into comprehensive programming which have been incorporated into new tobacco initiatives. As well, he serves as a consultant to numerous health units, public agencies, and businesses on the design and implementation of tobacco control programs. Dr. Abernathy is appointed as Associate Professor in the Department of Preventive Medicine and Biostatistics at the University of Toronto.

Dr. Mary Jane Ashley is a Principal Investigator in the Ontario Tobacco Research Unit. Trained in Medicine (MD), Epidemiology (MSC) and Public Health (DPH) she holds the rank of Professor in the Department of Preventive Medicine and Biostatistics at the University of Toronto. She has conducted research and policy work in the tobacco field for more than 20 years. Her research includes work on the roles of health professionals, the health effects of smoking, and short and long term evaluations of the City of Toronto Workplace Smoking Bylaw. She has served on many task forces and committees, including the Task Force of the Canadian Public Health Association on Tobacco Policy, the Expert Committee on Passive Smoking of Health and Welfare Canada, and the Task Force on Smoking of the Ontario Council of Health.

Dr. Stephen K. Brown, Ph.D., Professor and Chair, Department of Statistics and Actuarial Science, University of Waterloo. Co-Director, Waterloo Smoking Projects. Dr. Brown's expertise is in study design, longitudinal data analysis, and evaluation. He was co-investigator on a national survey of more than 100,000 Canadian school children. He has been co-investigator on three major longitudinal studies of the effectiveness of a social influences smoking prevention curriculum conducted by the Waterloo Smoking Projects. Dr. Brown has also served as a consultant to the Hunter Region Smoking Prevention Study, Newcastle, Australia, and the Hutchinson Smoking Prevention Program, Fred Hutchinson Cancer Research Centre, Seattle. He has also been a co-investigator on national and provincial surveys of smoking curricula used by schools and school boards. Other smoking related projects include the investigation of ETS constituents in air samples from homes, cross-national modelling of lung cancer rates and cigarette consumption data, and synthesis of literature on smoking prevention.

Dr. Roberta Ferrence is Director of the Ontario Tobacco Research Unit and Senior Scientist with the Addiction Research Foundation of Ontario. She holds the rank of Associate Professor in the Department of Preventive Medicine and Biostatistics at the University of Toronto. Trained in Medical Sociology (M.A.) and Epidemiology (PhD.), Dr. Ferrence has had extensive experience in epidemiologic and survey research in the addictions field. She has served in an expert capacity on numerous task forces and committees. Much of her work has concerned tobacco use and

tobacco control strategies, including policy and programming papers and reports. Of particular note is recent work dealing with: attitudes of the population of Ontario toward tobacco control policy; assess of minors to tobacco; tobacco taxation policy; analyses of trends in long-term use and patterns of current tobacco use; gender differences in tobacco use; the promotion of smoke-free families; nicotine dependence; and relationships among alcohol, tobacco and other health behaviours. As a volunteer professional, she currently chairs the Task Force on Smoking for the Heart and Stroke Foundation of Ontario.

Dr. Elizabeth Lindsay has a Ph.D. in Epidemiology and Preventive Medicine and holds the rank of Associate Professor in the Community Health Research Unit at the University of Ottawa. She is a Principal Investigator in the Ontario Tobacco Research Unit. Besides earlier research work with Dr. Douglas Wilson on ways of improving the effectiveness of family practitioners in smoking cessation, in recent years she has been a member of the COMMIT research group which has studied the roles of physicians and dentists in tobacco control, particularly their office practices, the influence of various strategies on practices, such as specific training, and the barriers in practice settings that impede effectiveness in performance of roles. She has much experience in conducting research in physician groups. She can access the COMMIT researcher network for advice on any aspect of this project that might be useful.

Dr. Linda Pederson holds the rank of Professor in the Department of Epidemiology and Biostatistics at the University of Western Ontario. She is a principal Investigator in the Ontario Tobacco Research Unit. Dr. Pederson has more than 15 years of research experience in tobacco. Among her tobacco research undertakings are several surveys of the population of Ontario dealing with knowledge of the health effects of tobacco, attitudes to restrictive measures and other tobacco control policies, and predicted compliance with more restrictions, a short and long-term evaluation of the City of Toronto workplace smoking by-law, studies of the determinants and natural history of cigarette smoking among young people, analysis of national and provincial health surveys concerning policy and other determinants of tobacco use, strategies for the factors related to successful cessation of smoking in medical settings, and aids to cessation. Of particular note among her literature reviews on various aspects of smoking is the chapter she contributed to the 1987 US Surgeon-General's Report on the Health Consequences of Smoking entitled "Role of the Physician in Smoking Cessation". Recently, she served on the Advisory Board regarding the development of "Guide Your Patients to a Smoke Free Future". Dr. Pederson has a PhD in Epidemiology and was a National Health Scholar under the NHRDP for 10 years.

Dr. Irving Rootman is the Director of the Centre for Health Promotion at the University of Toronto. He has a Ph.D. in sociology from Yale University and has been active in the field of health for more than 25 years, 17 of which were with Health and Welfare Canada, where among other things, he directed a division responsible for research, education and training, communications and workplace progress. Among his responsibilities was directing the implementation of an evaluation framework for the Health Promotion Program.

APPENDIX D

Literature Search Data Base Evaluations Summary

